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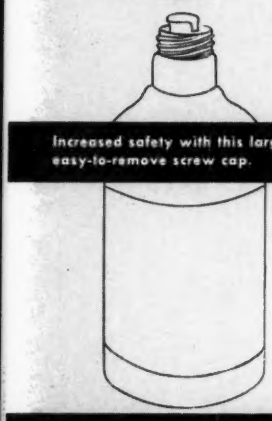

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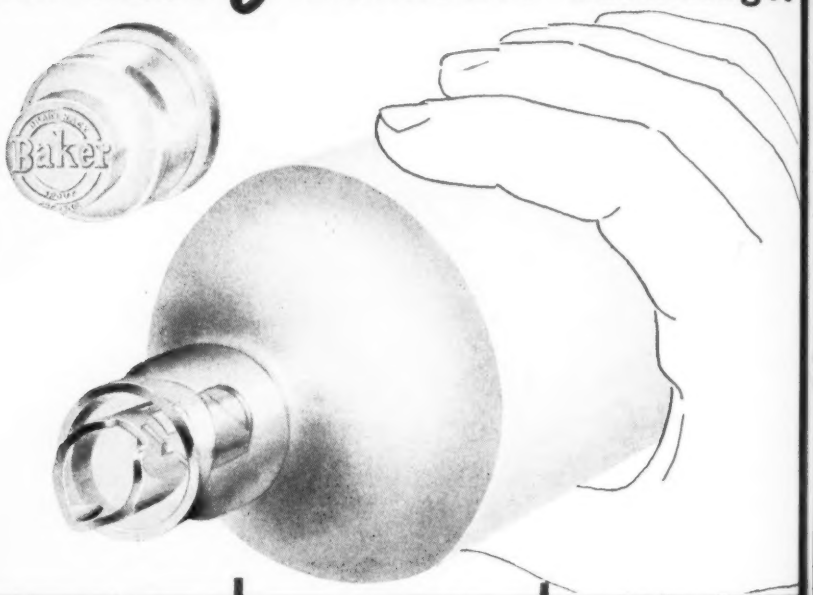

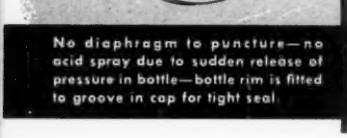

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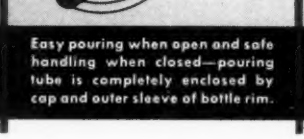

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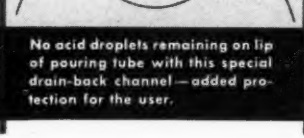

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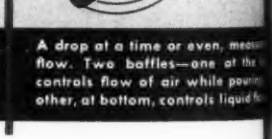

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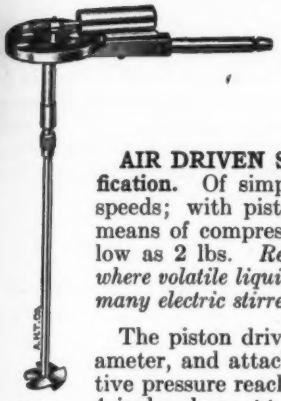

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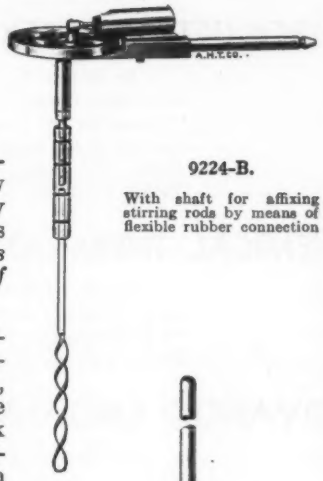

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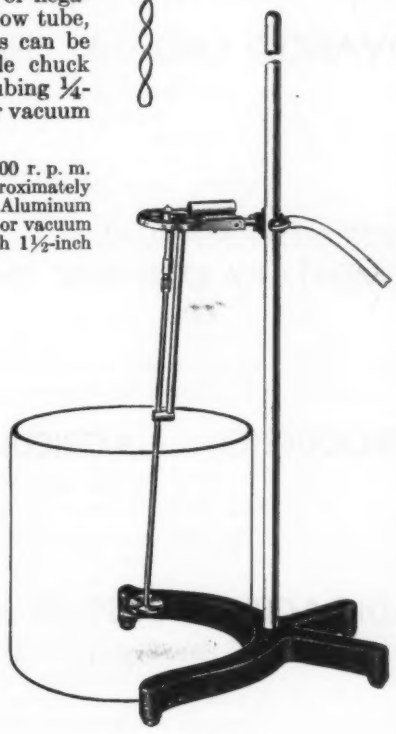

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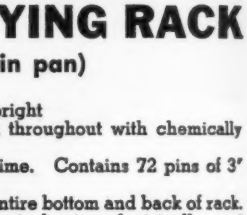

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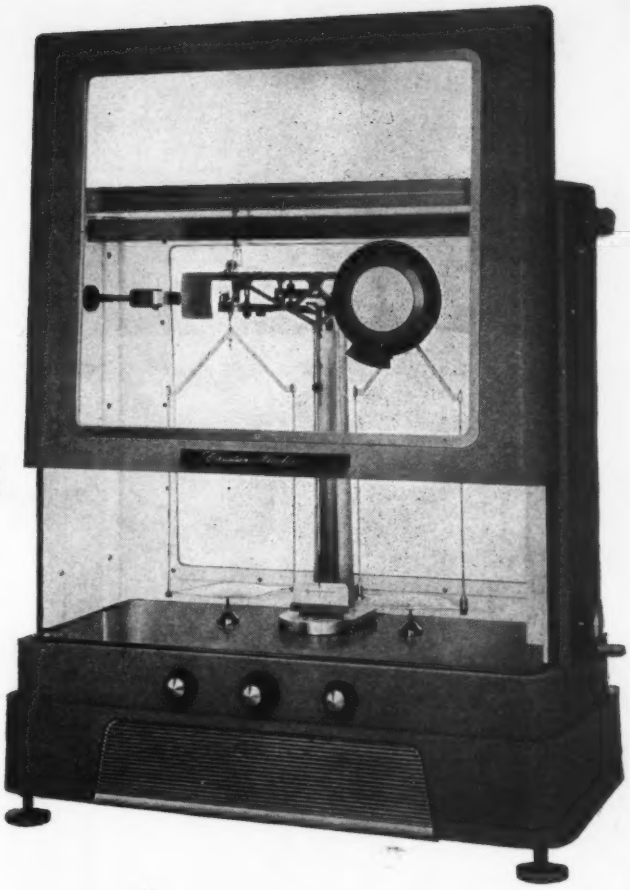

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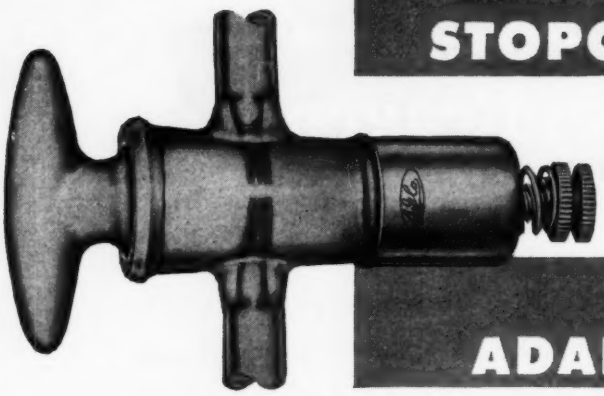

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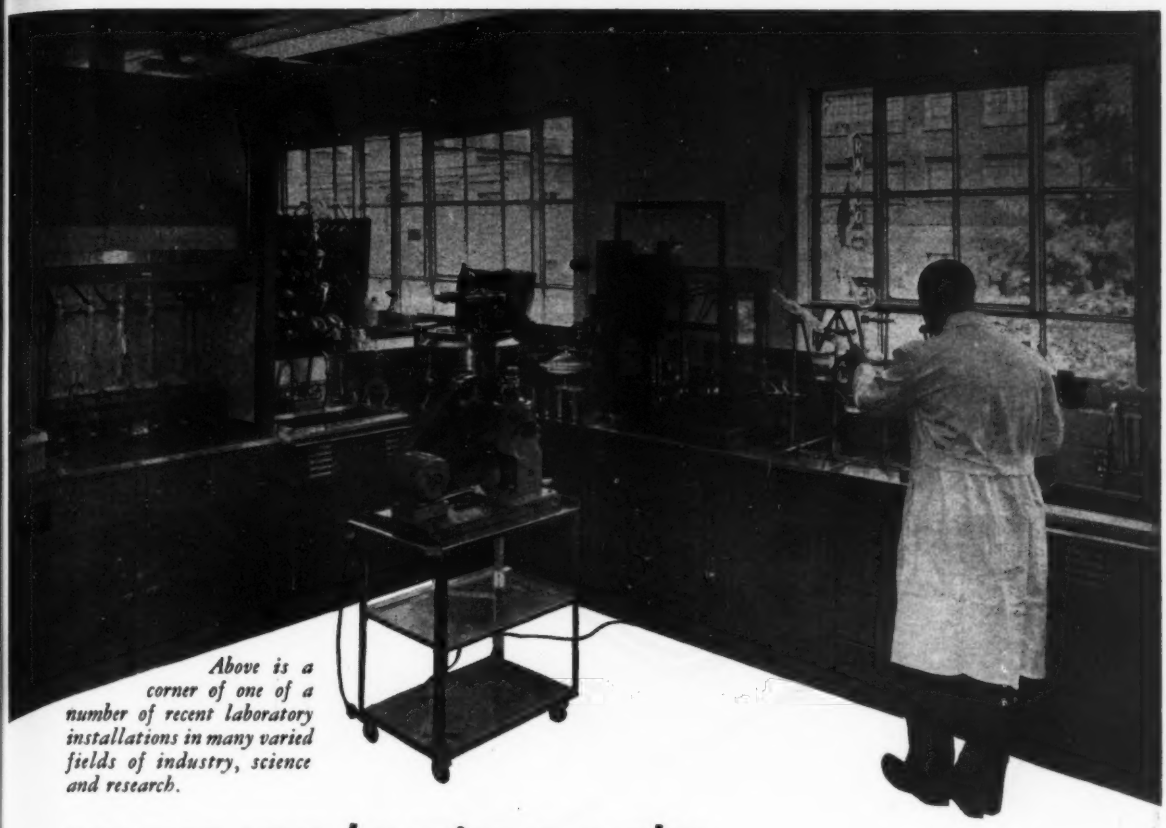

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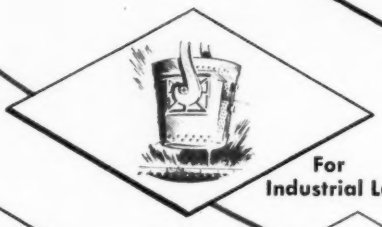

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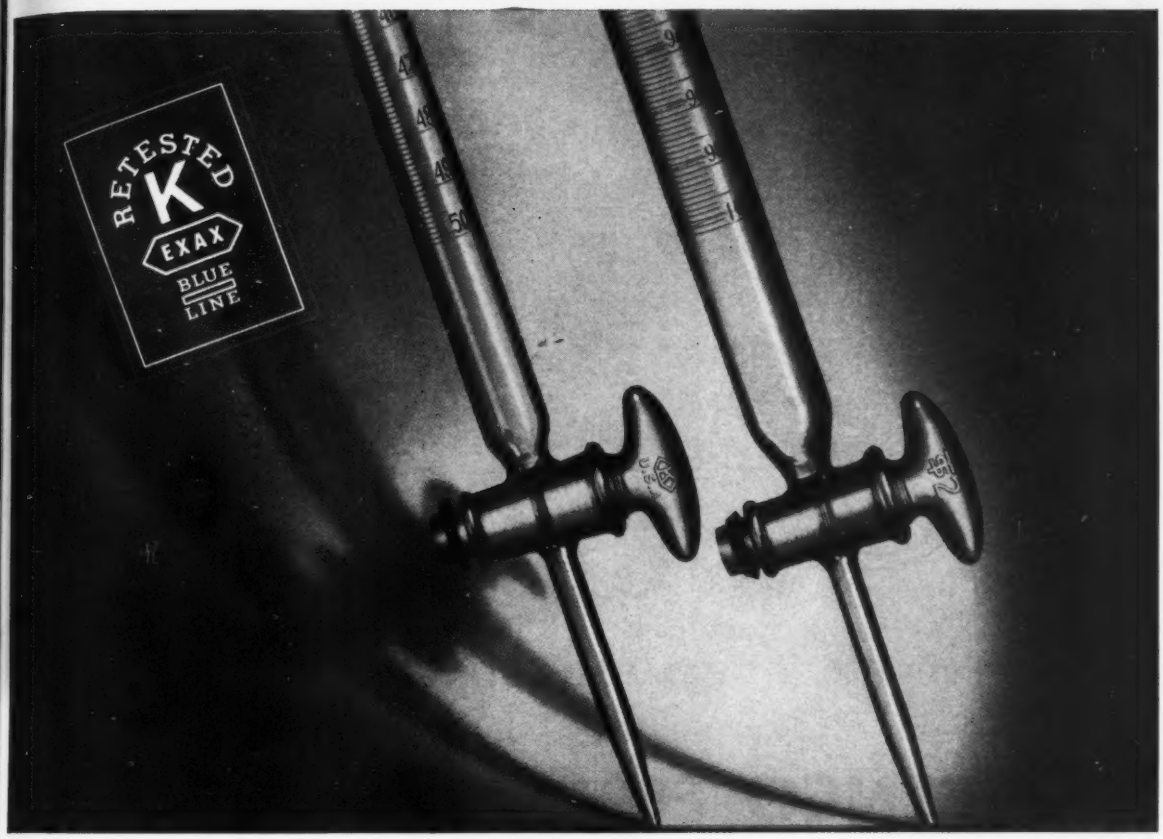

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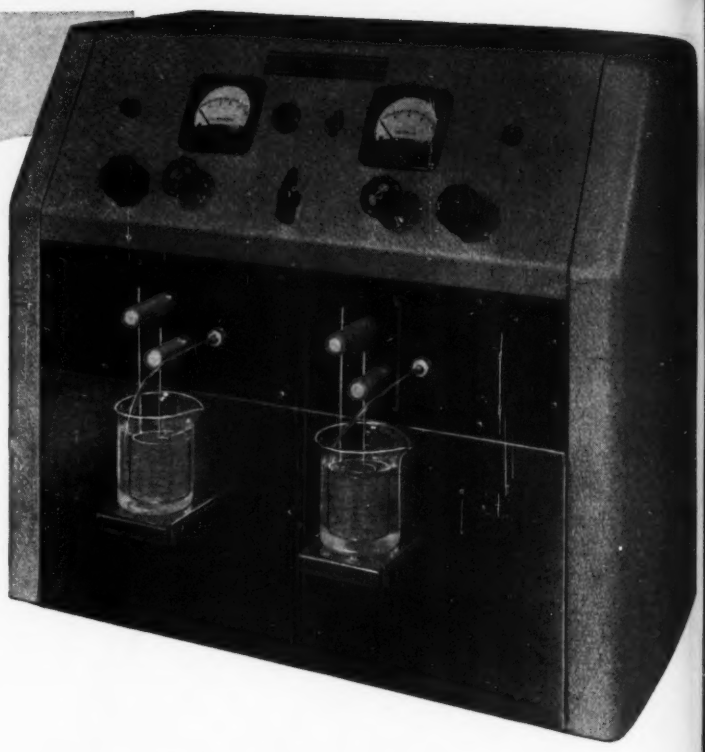

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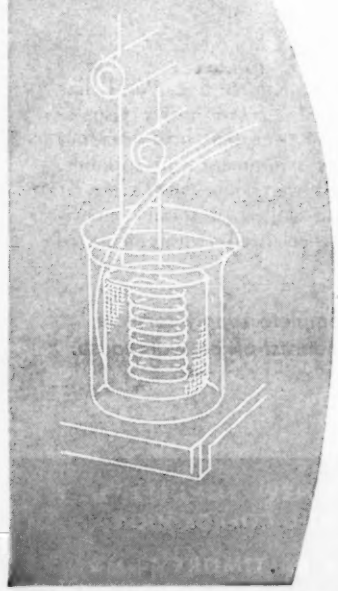

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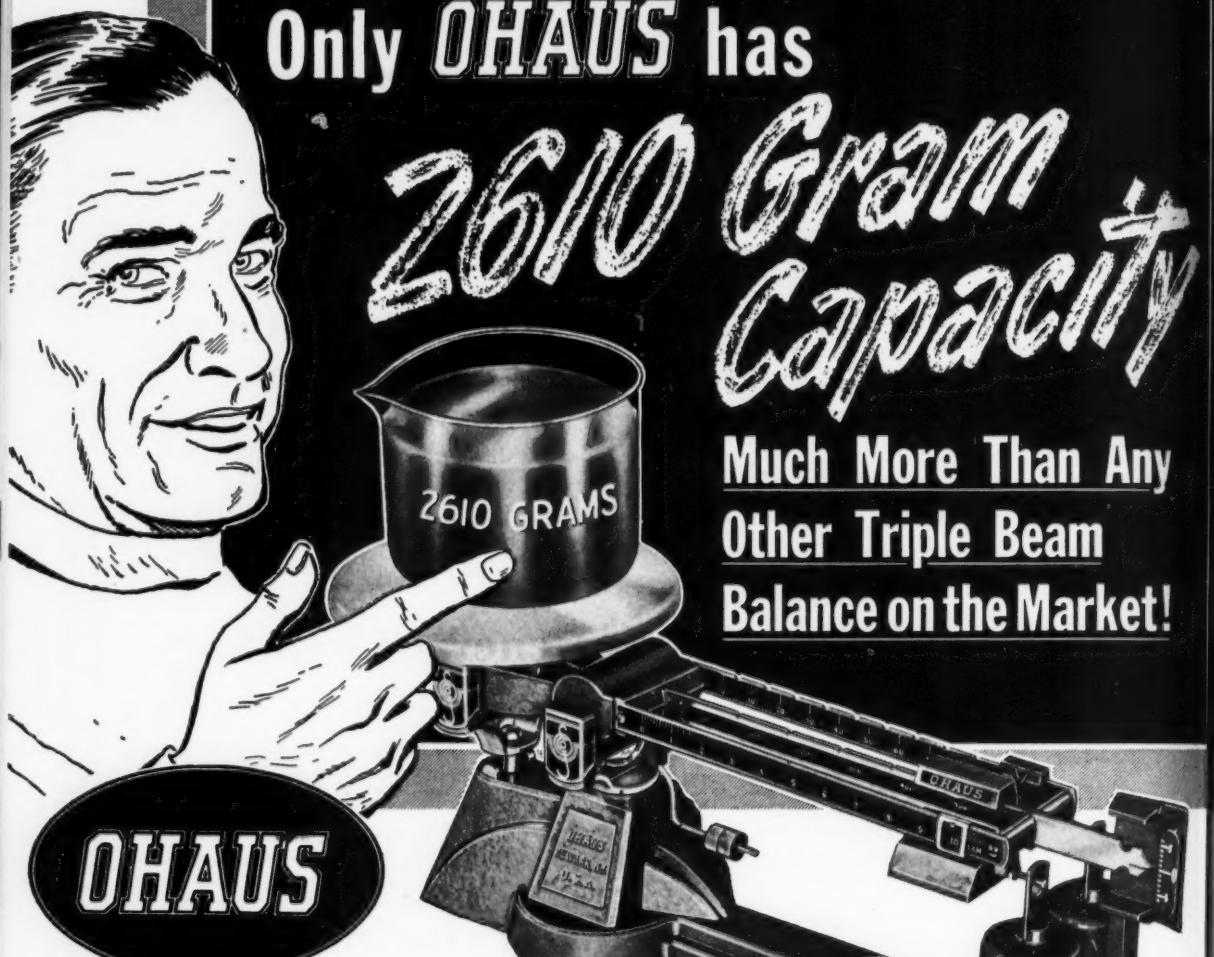

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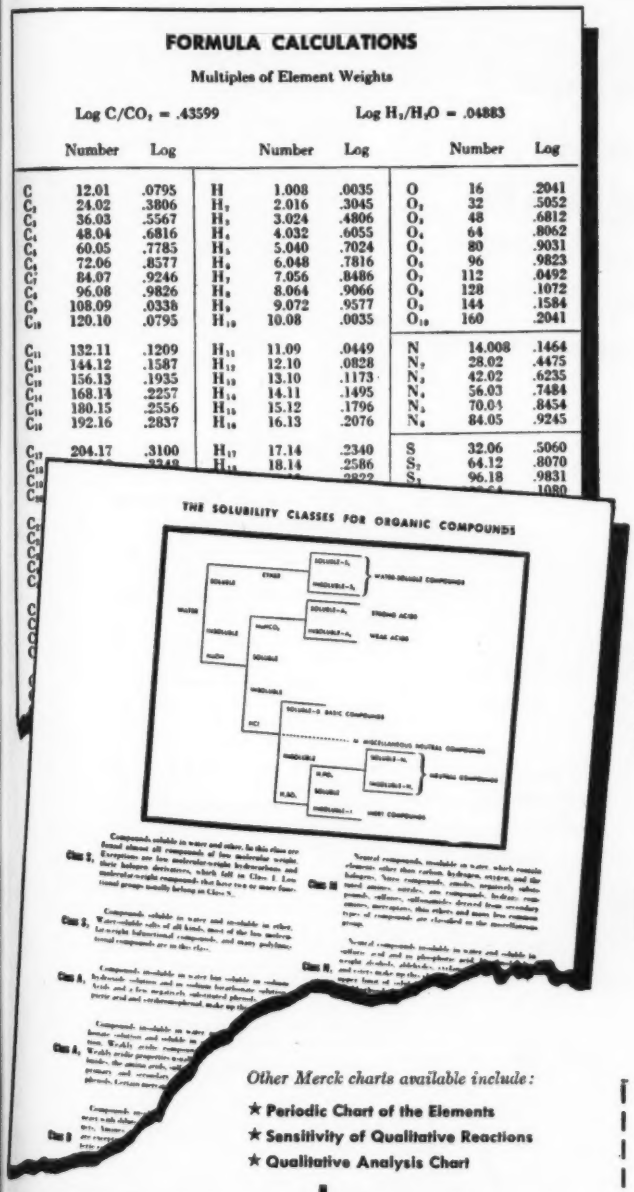

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Multiples of Element Weights

Log C/CO_2 = .43599 Log H_2/H_2O = .04883

	Number	Log		Number	Log		Number	Log
C	12.01	.0795	H	1.008	.0035	O	16	.2041
C_2	24.02	.3806	H_2	2.016	.3045	O_2	32	.5052
C_3	36.03	.5567	H_3	3.024	.4806	O_3	48	.6812
C_4	48.04	.6816	H_4	4.032	.6055	O_4	64	.8062
C_5	60.05	.7785	H_5	5.040	.7024	O_5	80	.9031
C_6	72.06	.8577	H_6	6.048	.7816	O_6	96	.9823
C_7	84.07	.9246	H_7	7.056	.8486	O_7	112	.0492
C_8	96.08	.9826	H_8	8.064	.9066	O_8	128	.1072
C_9	108.09	.0338	H_9	9.072	.9577	O_9	144	.1584
C_{10}	120.10	.0795	H_{10}	10.08	.0035	O_{10}	160	.2041
C_{11}	132.11	.1209	H_{11}	11.09	.0449	N	14.008	.1464
C_{12}	144.12	.1587	H_{12}	12.10	.0828	N_2	28.02	.4475
C_{13}	156.13	.1935	H_{13}	13.10	.1173	N_3	42.02	.6235
C_{14}	168.14	.2257	H_{14}	14.11	.1495	N_4	56.03	.7484
C_{15}	180.15	.2556	H_{15}	15.12	.1796	N_5	70.04	.8454
C_{16}	192.16	.2837	H_{16}	16.13	.2076	N_6	84.05	.9245
C_{17}	204.17	.3100	H_{17}	17.14	.2340	S	32.06	.5060
C_{18}			H_{18}	18.14	.2586	S_2	64.12	.8070
C_{19}						S_3	96.18	.9831
C_{20}								.1080

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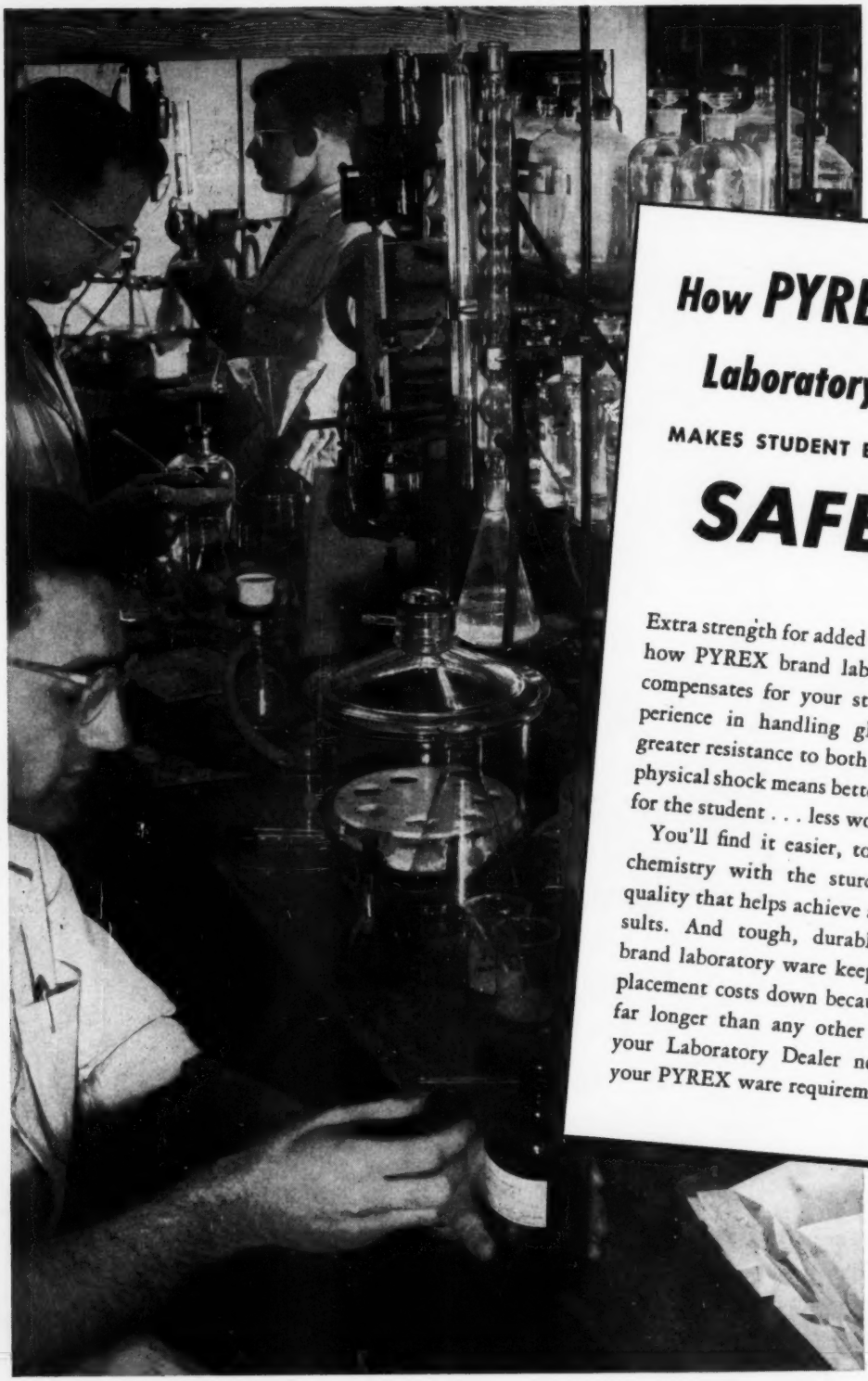

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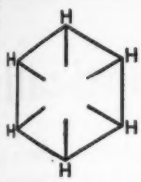

FIG. 75

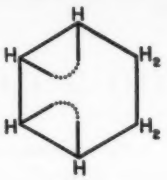

FIG. 76

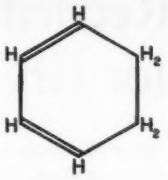

FIG. 77

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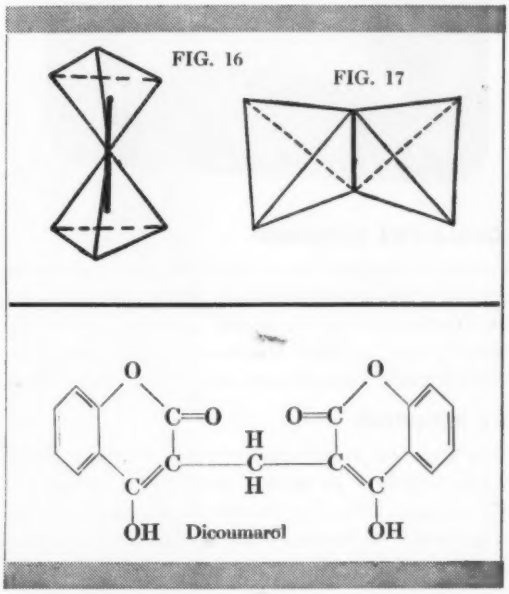

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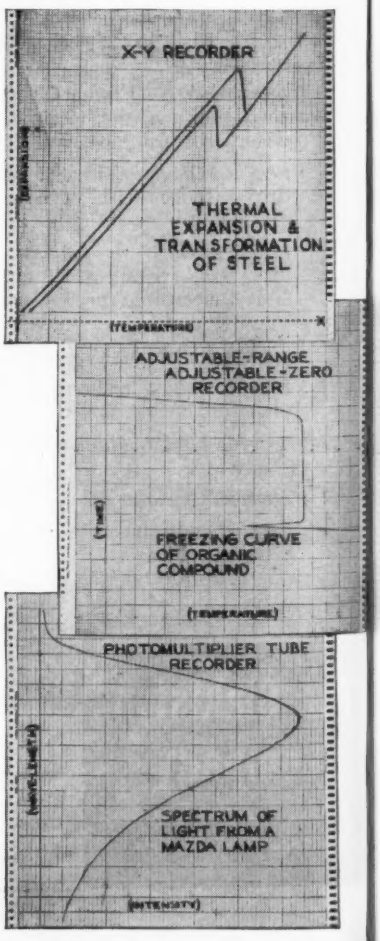
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(See page 120)

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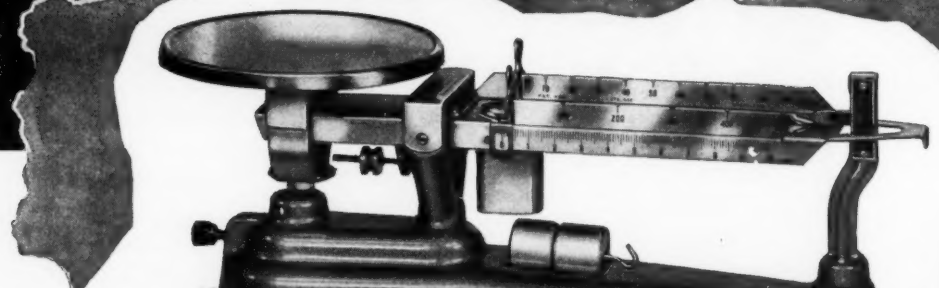

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Editor's Outlook

In any discussion of teaching, the overworked lecture method comes in for its share of castigation. A slightly new approach to this whole matter was made recently by Professor Laurence E. Dodd in an article entitled, "The old-time classroom recitation: can it be restored?"[1] He says:

> Nowadays, particularly in lower division physics, the restriction of opportunity for self-expression in the classroom goes along with the great emphasis that is placed on the lecture method. This method, in the writer's opinion, is admittedly valuable, even indispensable. However, regarding the whole physics-teaching program, there would seem to be a present over-emphasis on this one method, an over-emphasis created by large enrollments, together both with limited facilities to cope with them, and with the large amount of material to be studied.
>
> As to the latter, are we proceeding too fast with what may be called the "sifting-down" process? In this particular process, affecting especially today undergraduate teaching, topics that not so many years ago appeared only among research workers and in graduate seminars have been boiled down to simple forms that are finding their way into the general physics textbooks. This process is natural and inevitable. At the lower division level, is it proceeding too rapidly? Or, is it simply unavoidable?

The really constructive point is that his conception of the "recitation" is not what many of us may have in mind as the "old-time" picture. He goes on:

> Our main thesis is that any disused teaching method whereby the student could be helped effectively toward greater individual development, should be restored. If it should be, then we have a right to think it can be.
>
> At the college level, when classes are limited to the lecture and the laboratory, a certain lack of balance develops. This is detrimental to the student, not only in that he gets less practice in oral self-expression, but also because his view of things in the classroom and to some degree in life becomes lopsided. There is missing the salutary discipline in which he himself, rather than the lecturer, is occasionally the star performer. The regular lecturer may on his part come to feel that he has, in the eyes of the class and in his own eyes, too much the character of a paid actor, critically watched by his audience for any possible slip in the performance. Thus among the students there can result a superficially critical, but in reality uncritical, attitude—from the standpoint of learning the principles of physics—that is too easily satisfied with what might be only a superficially appealing performance. They look for an uninterrupted flow of words and a smooth manipulation of apparatus. Only if student self-criticism is aroused adequately, is a sound balance in teaching attained. This desideratum the best aspects of the old-time recitation supplied.

If the purpose of the recitation is not merely to "recite" but to make a considered, personal contribution to the discussion of a subject it deserves to be emphasized as an important part of educational experience—at any stage in the educational process.

We can conclude with Professor Dodd:

> "It seems clear that in our large classes we need more compulsion toward oral participation by the individual student, a broader and deeper stimulus to individual endeavor, to a greater ability, through practice, in accurate oral self-expression.

[1] *Am. J. Phys.*, 19, 14 (1951).

HERMANN STAUDINGER

Thirty Years of Macromolecules

WILLEM QUARLES
Sveavägen 120, Stockholm, Sweden

One of the last great organic chemists of the old school, Professor Hermann Staudinger, director of the Institute for Macromolecular Chemistry at Freiburg in Breisgau, Germany, celebrates his 70th birthday on March 23rd. This date is remarkable, as it will be exactly 30 years since he started his research on high molecular compounds, which was to become his lifework. With his unfailing energy, eminent pedagogic qualities, and his sharp pen he succeeded in winning an international group of scientists for his bold ideas, and it is certainly to a great extent due to Staudinger's perseverance and enthusiasm that the chemistry of "high polymers" developed at such an astonishing rate and within a few decades became a world-wide industry.

After having studied at the Universities of Halle, München, and Darmstadt, Staudinger took his Doctor's degree in 1907 under Thiele at Strasbourg and in the same year became professor at Karlsruhe. One of his first objects of research was the preparation of compounds with trivalent carbon atoms. He treated diphenylchloroacetyl chloride with zinc and to his surprise obtained a hitherto unknown compound, diphenyl ketene, which through its great reactivity became the starting point of a wide field of research.

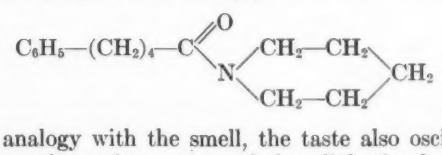
In 1912 Staudinger published his first book on these investigations called "Die Ketene," which is still considered a standard work. During his experiments with ketenes he noted that organic chlorine compounds and alkali metals form remarkably unstable systems, which are even more sensitive than fulminate of mercury but do not explode on heating. Staudinger's students remember, not always with pleasure, the master's love of detonations. Whenever possible the lectures were accompanied by bangs from some unsaturated hydrocarbon.

In the same year Staudinger moved to Zürich as Professor of Organic Chemistry, where he soon attracted an international crowd of students including some from the United States. They all came under the spell of this tall German who, with his curt nervous gestures and whispering voice, penetrated into the mysteries of the carbon atom and kindled a love for the strange organic world.

His first task as a teacher was the compilation of an adequate manual for the ordinary analysis of organic mixtures and compounds, which as yet did not exist.

This work would give the student a far better insight into the mechanism of organic reactions than the syntheses of simple compounds as practiced hitherto. The little book seemed to supply a want, as it was soon translated into English, French, Spanish, and even Japanese.

When looking for objects of scientific research Staudinger kept in close contact with the chemical industry, which especially during the first world war had to deal with a number of pressing problems. Together with Ruzicka he investigated the constitution of the insecticide Pyrethrine and tried to find a substitute for pepper. During this research he especially investigated the relation between taste and chemical constitution and found that the taste of pepper originated from an aralkylacidamide of piperidine:



In analogy with the smell, the taste also oscillated between the carbon atoms of the aliphatic chain, so that compounds with two, four, and six CH_2 groups had sharper pepper taste than those with one, three, and five CH_2 groups. The best pepper substitute proved to be *d*-phenyl valerianic acid piperidine, which compound was manufactured in Germany during both world wars.

A third problem Staudinger attacked was the constitution of the flavor of coffee, which presumably resided in a few well-defined compounds. This investigation was performed by one of his assistants, Thadeus Reichstein, who recently received the Nobel Prize.

It was not easy for him to keep his coffee research secret, as during many months the west wing of the old "chemicum" in Zürich was wrapped in a penetrating odor of mocha, which surpassed all other characteristic laboratory smells. He found that the flavor was largely due to furfuryl mercaptan mixed with a number of ketones and aldehydes. The preparation of the former compound was patented in several countries and manufactured on a large scale during World War II. It has of late also been put on the market by Cargille Scientific Inc., New York.

All these interesting investigations did not, however, divert Staudinger from his old hobby, the ketenes, especially when he found that these unsaturated com-

pounds could yield polymerization products with stable four-ring cyclobutane derivatives containing ketone and hydroxyl groups. Shortly after publishing this discovery Staudinger was attacked by Professor Schroeter of the veterinary college at Berlin, who proved that dimer ketenes did not form four-membered rings at all. He had condensed acetone dicarbonic acid ester and obtained a "cyclobutane," which was not identical with the product made by Staudinger. The 34-year-old Zürich professor however had his answer ready and by a series of brilliant experiments produced the incontestable proof that Schroeter was wrong and that his condensation product was not, as he thought, cyclobutanedione but the enol compound, cyclobutenolon. The first was fluid and volatile and could be methylated to a camphor-smelling derivative, while the latter, containing a hydroxyl group, was acid and could be titrated with carbonates. He finally succeeded in preparing two stereoisomeric cyclobutane compounds by polymerizing asymmetric camphoketenes.

This seemingly unimportant skirmish on polymerization was the beginning of a chemical battle which would last over a quarter of a century, and which Staudinger, thanks to his tenacy and ingenuity, turned into one of the most interesting chapters of modern organic chemistry.

In the years around 1910, Harries published his theories on the constitution of rubber, a problem which was at that time the topic of the day in Germany. As early as 1875 the Frenchman, Bouchardat, found that isoprene could be polymerized to a rubber-like substance, while Harries, from a series of reactions with ozone, concluded that rubber consisted of rings of dimeric isoprene molecules. In 1913 the first artificial rubber made its appearance in Germany, and in view of the war menace the German chemists concentrated all efforts to produce isoprene on a technical scale as cheaply as possible.

With his knowledge of the behavior of four-membered ring compounds and their aptitude to break up into unsaturated parts, Staudinger hoped that dipentene would yield two molecules of isoprene, which indeed proved to be the case. By diluting the cracking vapors with indifferent gases he was able to raise the output to over 60 per cent.

Staudinger was not satisfied with Harries' rubber theory, and this problem occupied him nearly ten years. The Röntgen diagram of stretched rubber showed a definite fiber structure, which gave him the idea that the isoprene molecules formed long straight chains. This hypothesis put him in direct opposition to the current opinion, represented by Karrer, Hess, K. H. Meyer, and others, according to whom the macromolecular substances like rubber, cellulose, starch, etc., did not consist of large molecules but of conglomerations of minor "base molecules," kept together by van der Waals forces. If the chain hypothesis was right it ought to be possible to hydrogenate rubber to a paraffin-like material. After years of investigation Staudinger actually succeeded in carrying out this reaction with hydrogen and a nickel catalyst at 270°C. and 100 atm. pressure. He obtained a saturated high-molecular hydrocarbon with typical paraffin-like properties, which, only under high vacuum and above 350°C., was split up into unsaturated fragments. Staudinger published the result in *Helvetica Chimica Acta* No. 5 (1922), of which page 788 has become an historic document, as there the term "Makromolekel" was used for the first time.

Despite these convincing experiments, the chain theory won Staudinger a large number of opponents, who objected that the presence of unsaturated final groups in the molecules made the formation of rings imperative. According to Staudinger the influence of these unsaturated groups vanished in the mass of the giant molecules, containing 2 to 3000 isoprene links. The hydrogenation of the rubber proved, moreover, that vulcanization was not an absorption phenomenon in a colloidal rubber solution, but a pure chemical reaction, as the hydrorubber did not react with sulfur.

The publication constituted the first step on the long road leading to a better understanding of the giant molecules, which are found everywhere in the organic world, and which the scientists, as Svedberg says, "had thrown into the lumber room because they did not know what to do with them."

In order to avoid inconsistency in the results when working with natural products like rubber, cellulose, etc., Staudinger preferred to carry out his research with "model" substances, synthetic giant molecules, prepared from simple compounds like formaldehyde or styrol, which yielded homogeneous polymerization products. Röntgenographic examination of the polyoxymethylenes obtained, and the colloidal properties of polystyrols, disproved clearly the popular "micelle" theory of high polymers. Staudinger finally succeeded in subjecting his polymerization products to certain chemical reactions, which, to his great satisfaction, did not affect the degree of polymerization. These "polymer-analogous" reactions were exceedingly difficult to perform, and Staudinger himself considers the discovery of them one of his most important contributions to the chemistry of macromolecules, *e. g.*, the acetylation of cellulose, the reduction of polystyrol to hexahydrostyrol, etc. At the "Naturforscher Congress" at Düsseldorf in 1926 Staudinger read his first paper on the results of his investigations in this field and proposed a number of revolutionary theories.

He found himself quite isolated, however, in face of a compact opposition. Only one colleague, Richard Willstätter, understood the importance of Staudinger's polymer-analogous reactions as a proof of the soundness of his ideas. Two years later Meyer and Mark developed a new micelle theory according to which the colloidal parcels constituted conglomerations of chain molecules. In this way a number of previously incomprehensible phenomena could be elegantly explained. This led Staudinger to a new series of investigations. In the meantime, he had been called to Freiburg where

he has remained until now. In 1940 he founded the first Institute for Macromolecular Chemistry and started the first periodical in this special field.

The decennium 1926–36 constituted the height of Staudinger's life work. During this period he found a relation between the viscosity and the molecular weight of dilute solutions of macromolecules, which suggested a practical method of estimating the length of the chain and the degree of polymerization. This so-called "Staudinger Law of Viscosity" has been vehemently discussed, as with giant molecules all "regularities" imply a number of exceptions. In spite of all criticism it has not been possible to find a more practical way of measuring the unmanageable compounds, and "Staudinger Molecular Weight" is a current term in the plastic industry. In connection with this law, Staudinger classified the organic colloids into two groups according to the shape of the macromolecules: the *spheric* colloids, such as some glycogens, and the *linear* colloids, like cellulose. This distinction enables one to explain the fact that, in nature, certain high-molecular polysaccharides form low-viscosity solutions, while others with a low molecular weight produce solutions with a high viscosity.

It would be too much to relate all the important results which emanated from Staudingers' institute. They have been described in several hundred papers.

It is obvious that his many bold ideas, which were often contrary to all current conceptions, provided fertile working hypotheses and greatly stimulated the high polymer industry. One of his interesting discoveries of technical importance is the "bridging" of polystyrol chains by traces of divinyl benzene into insoluble products, which has become a well-known technical trick in the manufacture of plastics.

After 25 years of intensive research with a staff of collaborators, Staudinger succeeded in penetrating one of the most inaccessible fields of organic chemistry, lifting a corner of the veil covering the mysterious world of the giant molecules. He has summarized his lifework in three books: "Die Hochmolekularen Organischen Verbindungen" (1932), "Organische Kolloidchemie" (1941), and "Makromolekulare Chemie und Biologie" (1947).

THE USE OF STARCH-IODATE PAPER IN QUALITATIVE ANALYSIS

JOHN A. LUNDIN
Macalester College, St. Paul, Minnesota

IN EVERY class of semimicro qualitative analysis a large number of students seem to have trouble in distinguishing between the sulfite ion and the carbonate ion when gaseous methods are used for their detection. Many such methods have been developed which, *if* carried out according to directions, will detect each ion nicely. However, they do have some disadvantages. In the methods where the formation of Prussian blue is used to detect the sulfite ion, contamination from previous experiments may cause the decomposition of the ferric ferricyanide which many mistake for a positive test. The disadvantage of precipitating the two above ions as their calcium or barium salts lies in their identical physical appearance. In many other techniques the apparatus used is bulky and cumbersome.

A simple answer to the above problems proved to be the use of "starch-iodate" paper in conjunction with the above tests. The paper turns blue on contact with the sulfur dioxide.

The "starch-iodate" paper is prepared simply by moistening a piece of filter paper with a drop of each of the following solutions: starch, potassium iodate, and dilute nitric acid.

A few drops of the solution to be tested is placed in a 5-cm. test tube and acidified with dilute hydrochloric acid. The test paper is placed over the mouth of the test tube and the tube heated in a hot water bath.

The other anions which give gases upon acidification and interfere with the test are the sulfide and the thiosulfate ions. The sulfide ion may be removed easily by precipitating with cadmium nitrate and applying the test to the centrifugate. The presence of thiosulfate necessitates the removal of the sulfite ion with strontium nitrate and the testing of a portion of the precipitate.

As little as 0.3 mg. of sulfite ion can be detected by this method.

The "starch-iodate" paper may also be used as a preliminary test for the reducing anions. In this case, a drop of the test solution is merely placed upon the acidified paper. A positive test was obtained with the following anions: S^{--} (0.1), SO_3^{--} (0.1), $S_2O_3^{--}$ (10.0), AsO_3^{---} (1.0), $Fe(CN)_6^{----}$ (10.0), I^- (0.1), NO_2^- (10.0), and SCN^- (0.05). (The number in the parentheses indicates the number of milligrams of ion per milliliter of solution.)

THE ORIGINS OF BEER'S LAW

HEINZ G. PFEIFFER and HERMAN A. LIEBHAFSKY
General Electric Company, Schenectady, New York

THE examination of many books and articles dealing with photometric analysis had given us the impression that Beer discovered and formulated the exponential absorption law in which concentration and length appear as symmetrical variables. We were led to carry out this examination because of our belief[1] that an absorption law so formulated is too restricted for the needs of modern analytical chemistry. When we read Beer's article,[2] we were surprised to find this impression wrong in several important respects, and we have consequently prepared the following discussion of his classical investigation. Of the more recent publications we examined, an article by Hague[3] comes closest in our estimation to Beer's point of view.

We shall begin by giving a free (but, we hope, scientifically accurate) translation of part of Beer's paper. Interpolated parenthetical material and italics are our own. Equations have been numbered in sequence and some of Beer's symbols are explained later. On page 83, Beer says:

> We shall take the *absorption coefficient*[4] to be the coefficient giving the diminution in amplitude suffered by a light ray as it passes through unit length of an absorbing material. The unit of length will be one decimeter. We then have, according to theory, and as I have found verified by experiment,
>
> $$\lambda = \mu^D \tag{1}$$
>
> where μ is the absorption coefficient and D the length of the absorbing material traversed in the experiment. Finally, we have for the absorption coefficient expressed in terms of quantities actually measured
>
> $$\mu = [\text{tang } \alpha/0.95 \text{ tang } \psi]^{1/D} \tag{2}$$

Beer's description of the theory and operation of his photometer agrees in every important detail with a modern description[5] of the method in which polarization is used to measure the relative amplitudes of an unknown and a reference beam. Beer used two oil lamps, carefully adjusted, and red glass as filter. Nicol prisms served as polarizers. In his apparatus, measurements of an angle gave the relative amplitudes of the two beams before and after the absorber was inserted in the unknown beam. Let the former value of the angle be ψ, and the latter α; then, in Beer's symbols

$$a/A = \text{tang } \psi \tag{3}$$

and

$$\nu a/A = \text{tang } \alpha \tag{4}$$

where a and A are the amplitudes of the unknown and of the reference beam before the absorber is inserted; and ν is a coefficient that gives the diminution in amplitude produced by the absorber—in Beer's case, an aqueous solution of a colored salt contained in a glass cell. From the refractive indices of glass and solution, Beer calculates that about five per cent of this diminution is due to losses at the cell windows; hence he writes

$$\lambda = \nu/r = \text{tang } \alpha/0.95 \text{ tang } \psi \tag{5}$$

where $r = 0.95$ corrects for these losses and ν takes its value from equations (1) and (2). The derivation of equation (2) is now clear.

To discuss equation (1) in modern terms, we should deal with the internal transmittance of the solution in Beer's cell. Hence, we write

$$\log [I_0/I] = a_m cb \tag{6}$$

where I_0 is the homogeneous radiant energy entering the sample, and I that incident upon its second surface[6]; c is the concentration in moles/liter; and b the length of solution in cm. Inasmuch as only ratios of I and of a are involved, we may write

$$I_0/I = (a/\lambda a)^2 = 1/\lambda^2 = 1/\mu^{2D} \tag{7}$$

hence

$$a_m cb = \log [1/\mu^{2D}] = -2D \log \mu \tag{8}$$

Also, owing to the different units of length,

$$D = 0.1b$$

or,

$$a_m c = -0.2 \log \mu \tag{9}$$

Equations 7 to 9 show conclusively that Beer did not treat concentration and length as symmetrical variables. Nowhere in his paper does he give any indication of being familiar with equation (6) in this or an alternative form; all his thinking seems to have centered around only one absorption law, equation (1), and (as will appear from the following translated ex-

[1] LIEBHAFSKY, HERMAN A., paper presented at the Pittsburgh Conference on Analytical Chemistry and Applied Spectroscopy, Pittsburgh, Pennsylvania, February 15 to 17, 1950.

[2] BEER, A., *Ann. Physik* (Poggendorf), **86**, 78 (1852).

[3] HAGUE, JOHN L., *Proc. Am. Soc. Testing Materials*, **44**, 712 (1944).

[4] Here Beer interpolates the Greek phrase κατ εξοχην which we have been told is roughly equivalent to *"par excellence."* We take this to mean that Beer is thinking of a true absorption coefficient; *i. e.*, with all corrections (such as cell corrections) applied.

[5] KOHLRAUSCH, F., "Praktische Physik," 18th ed., B. G. Teubner, Leipzig, 1943, Vol. 1, pp. 503–504.

[6] National Bureau of Standards, Washington, Letter Circular LC857.

cerpts) all his results were calculated on the basis of this law. Now let us see how Beer describes his experiments.

In most of my experiments, solutions diluted to various extents were employed. From the values of λ, which these experiments yielded, the absorption coefficient of 1 decimeter of the most concentrated solution (I) was calculated on the supposition *that the water added during the dilution did not alter the specific absorbing power* inasmuch as it exerts no chemical action and as its own absorption could be neglected. But that the diminution of light actually *depends only on the thickness of concentrated solution* (I) through which it passes is taught to us by observation. So, for example, a cell of 1 decimeter length was first filled with a solution of copper sulfate that contained 9 volumes of water for each volume of solution concentrated at 13.5°C. (II)—in other words a solution of dilution 1/9. Further, a cell was filled with a solution of dilution 1/19. The latter cell contains just so much concentrated solution (I or II) as the former; it has the same tint as the former in white light and gives sensibly the same value of α in red. The value of this angle was 3°28′10″ for the short cell and 3°23′0″ for the longer, and these yield the almost identical values 0.065 and 0.063 for the absorption coefficient of the concentrated solution (I).

In contrast with the rest of Beer's paper, this paragraph could confuse the unwary reader because "concentrated" is used in connection with both Beer's stock solution (II) and the strongest solution he actually measured (I). (We have taken the stock solution of the foregoing paragraph to be a solution of copper sulfate saturated at 13.5°C. although Beer's density data indicate it was only 98 per cent saturated.) Comparison of the values for the tangents of 3°28′10″ (0.061) and 3°23′0″ (0.059) with those just given for the absorption coefficients leads at once to the identification according to the Roman numerals given. In view of the fact that Beer tabulated values only for the most concentrated solution measured, it is probable that he was thinking of I where "I or II" appears in the preceding paragraph. At any rate, it is clear that Beer was thinking primarily of an *amount* (or thickness) of concentrated solution; in other words, of a *mass* of absorbing material, not of a *concentration*. To be logical, anyone who regards this as hair-splitting must also maintain that it is pointless to distinguish between extensive and intensive properties.

If there is no evidence of Beer's being familiar with an absorption law containing the concentration as an explicit variable, then how did he correlate his measurements, which were done at different concentrations? Fortunately, he answers this question in the following paragraph.

With copper sulfate as an example, it may further be shown how observations on a dilute solution lead to the absorption coefficient of one more concentrated. For a thickness of 1 decimeter and for the dilution 1/19, $\alpha = 13°48'10''$ was found for this salt. If λ is the corresponding coefficient of diminution (i. e., the corresponding transmissivity), then this coefficient will have the value λ^2 for double this thickness. But, at double this thickness, just so much concentrated solution will be irradiated as at a thickness of 1 decimeter and the dilution 1/9. Accordingly, λ^2 is the absorption coefficient for the dilution 1/9. One finds therefore the value 0.069, which is in satisfactory agreement with the values already given.

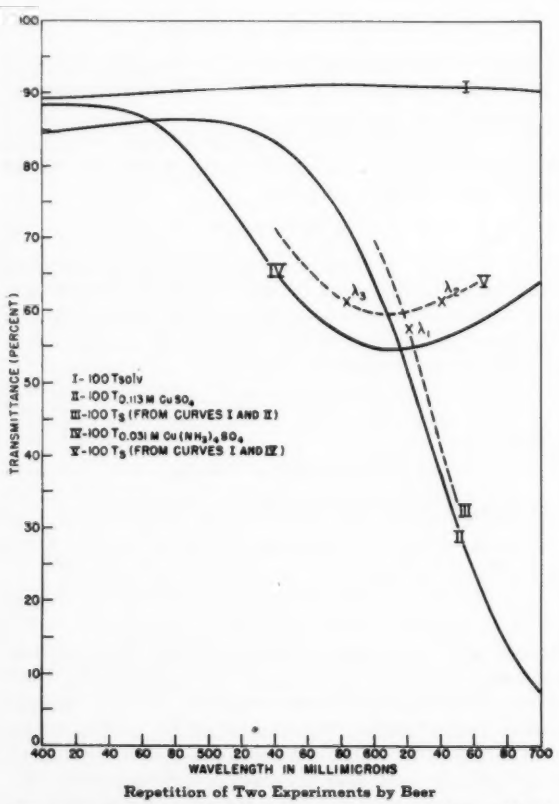
Finally, why did Beer carry out his investigation? He introduces his paper with these two sentences:

The absorption of light during the irradiation of a colored substance has often been the object of experiment; but attention has always been directed to the *relative* diminution of the various colors or, in the case of crystalline bodies, the relation between the absorption and the direction of polarization. Concerning the *absolute* magnitude of the absorption that a particular ray of light suffers during its propagation through an absorbing medium, there is—so far as I know—no information available.

The words "relative" and "absolute" deserve attention. Their use here confirms our belief that Beer did not know that concentration and length are symmetrical variables in equation (6). We believe he considered the "relative diminution" as expressed in equation (1) to be a function of the length of absorber, and that he consequently placed the length (D) in the exponent. We believe he may have thought that the change in the amount (or mass) of absorber produces an "absolute diminution" such that

$$\Delta I = -k(m) \qquad (10)$$

To summarize: (1) Beer's paper does not contain an absorption law in which concentration appears as an explicit variable; (2) Beer thought primarily of the



Repetition of Two Experiments by Beer

The agreement of λ_2 with λ_1 measures the internal consistency of Beer's work. The calculation cannot distinguish between λ_2 and λ_3, but it seems safe to ignore the latter. (T's for curves I, II, and IV are transmittances. T's for curves III and V are transmittancies. See reference 6.)

amount or mass of absorbing material; (3) Beer may or may not have understood the functional relationship between absorbance and mass (or concentration) of absorbing material; at any rate, he used length as an intermediate variable in correlating results obtained at different dilutions.

It is accordingly not surprising to find in Beer's paper no evidence that he believed himself to have discovered a new absorption law. In a thorough (but necessarily incomplete) investigation of the older literature, we found the phrase "Beer's Law" or "Beer's Absorption Law" first used by Walter[7] nearly forty years later. Kayser,[8] in his monumental "Handbuch der Spectroscopie" says: "If we let a represent the absorption coefficient for unit concentration, there follows the absorption law

$$I = I_o a^{dc} \qquad (11)$$

where d is the thickness of the layer and c the concentration. This law was set up by Beer and is called "Beer's Law." Comparison of equation (11) with equation (1) shows the first half of the last sentence to be incorrect. There is thus good precedent for any twentieth-century inaccuracies in describing what Beer thought and did.

We have made an attempt to evaluate the internal consistency of Beer's work. The figure gives transmittance curves measured by a General Electric Recording Spectrophotometer[9] on water and on two of Beer's solutions, all measurements being made in the same cell. Taking the differences in cell length into account, we have compared Beer's absorption coefficients with our measured *transmittancies* to locate the "effective wave length" (an X-ray term)[10] of Beer's red light. The figure shows these wave lengths to differ very little for the two solutions, which speaks highly for the quality of Beer's work.

The principal aim of this article has been to show that Beer, in thinking primarily of the *amount* of absorbing material, laid the foundation of an absorption law broader than equation (6). That he did not write this amount as an explicit variable in the absorption law seems to us no reason for not calling that law by his name, and his alone—after all, didn't Columbus discover America?

[7] WALTER, B., *Ann. Physik* (Wiedemann), **36**, 502, 518 (1889). See especially pp. 505, 512, 520, 521.

[8] KAYSER, H., "*Handbuch der Spectroscopie*," S. Hirzel, Leipzig, 1905, Vol. 3, pp. 10–25. We used the references there cited as a guide to the older literature.

[9] MICHAELSON, J. L., AND H. A. LIEBHAFSKY, *Gen. Elec. Rev.*, **39**, 445 (1936).

[10] See, for example, LIEBHAFSKY, HERMAN A., *Analytical Chemistry*, **21**, 19 (1949).

A SIMPLE MAGNETIC STIRRER

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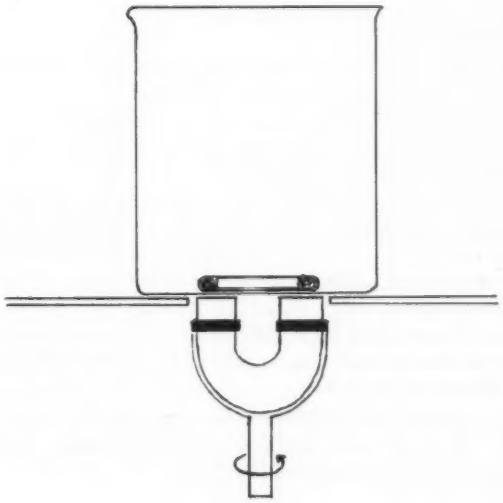
IN ONE phase of the preparation of an organic compound, the need of a stirrer in a closed system arose. Instead of a mercury-seal stirrer the following simple magnetic stirrer was devised. The stirrer was quite effective and could be useful in general laboratory preparative problems.

A small, very strong horseshoe Alnico magnet was wired onto a copper support. The support in turn was bolted to an inverted air stirrer.

The stirring effect in the case of a 600-ml. beaker is derived from the rotary motion given to an iron rod, sealed in glass, 1 in. in length and $^1/_4$ in. in diameter. The rod, on the inside of the beaker, follows the rotating magnet.

The support should be such that it allows the magnet to be placed close to the underside surface of the container.

A vortex of 1 in. in depth was easily produced in 400 ml. of water.



A NEW DERIVATIVE FOR THE IDENTIFICATION OF PRIMARY AROMATIC AMINES

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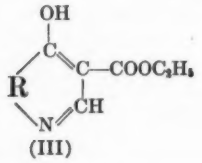
WHILE engaged in synthetic work involving the ethyl aminomethylenemalonates (II), the products of the reaction of ethyl ethoxymethylenemalonate (I) with primary aromatic amines, the author was impressed by

$$R{-}NH_2 + C_2H_5O{-}CH{=}C{-}(COOC_2H_5)_2 \rightarrow \underset{(I)}{} \quad \underset{(II)}{R{-}NH{-}CH{=}C{-}(COOC_2H_5)_2}$$



(III)

the ease with which these crystalline compounds were prepared and purified. They seemed to offer the possibility of being useful as derivatives for the identification of primary aromatic and heterocyclic amines, particularly for use on the semimicro and micro scale where isolation and purification of the more common benzoates and acetates present difficulties for the inexperienced student. Accordingly a procedure was developed and tried experimentally with five groups of students in qualitative organic analysis. In each case where the new derivative was applicable the student reported greater ease in preparation and purification as compared to other more common derivatives prepared. The chief advantages of the method were found to be:

1. Derivative could be prepared and purified in a shorter time than was usually possible with other derivatives.

2. No solvent was required, making it easier to isolate small quantities of the derivative.

3. No reaction or interference occurred with secondary or tertiary amines or with aliphatic primary amines under the conditions employed.

4. The derivative (II) can be readily cyclized to a second derivative (III) which has a readily hydrolyzable ester grouping, allowing for saponification-equivalent determination. The major disadvantages of the method were:

1. The reagent (EMME) is not commercially available at the present time. However, it can readily be prepared and keeps well.

2. Alkoxy and aryloxyanilines give oils.

3. Melting point differences between some similar compounds are not sufficiently great.

Preparation of EMME Derivative (II). Approximately equal weights of the amine and ethyl ethoxymethylenemalonate (I) were mixed in a small test tube and heated on the steam bath for fifteen to thirty minutes (nitroanilines require a temperature of about 130°) and cooled to room temperature. If necessary, crystallization was induced by scratching. The solid was removed and pressed between filter papers. It was then recrystallized from the minimum amount of hot ethanol, using charcoal if decolorization was required. One recrystallization ordinarily gave a sharp-melting derivative. The table lists the melting points of the derivatives of a number of amines.

Cyclization of II to III. For best results at least 0.5 g. of II should be available. To 2 ml. of boiling diphenyl ether was added 0.5 g. of II, boiling being continued for five to ten minutes. After cooling, the solution was diluted with 10 ml. of petroleum ether and the solid product was collected by suction filtration. Recrystallization from ethanol, pyridine, or a mixture of these gave a sharp melting compound. The table lists the melting points of a number of these derivatives.

Melting Points of Derivatives

Amine	*M. P. of II, °C., Uncorr.*	*M. P. of III, °C., Uncorr.*	*Reference*
p-Aminoacetophenone	93–34	298–300	4
p-Aminobenzoic acid	218–219	...	2
p-Aminobiphenyl	59–60	...	2
p-Aminophenol	115–116	...	2
2-Aminopyridine	65–66	Tar	1
8-Aminoquinoline	115–116	237–241	5
Aniline	49–50	269–270	4
o-Anisidine	Oil	274–275 dec.	...
p-Anisidine	Oil	276–277	3
5-Bromo-2-aminopyridine	117–118	134–135[a]	1
4-Chloroaniline	82–83	280	4
3-Chloroaniline	55–56	295–297	3
2-Chloroaniline	92–93	253–254	5
5-Chloro-2-aminopyridine	115–116	132–133[a]	1
2,4-Dichloroaniline	128–129	...	2
3,4-Dimethoxyaniline	Oil	272–273	4
p-Dimethylaminoaniline	97–98	270–275 dec.	4
3,4-Dimethylaniline	52–53.5	...	3
6-Methyl-2-aminopyridine	113–114	278–280[a]	1
5-Methyl-2-aminopyridine	112–113	135–136[a]	1
4-Methyl-2-aminopyridine	72–73	171–172[a]	1
4-Methoxy-2-nitroaniline	126–127	222–224	4
3-Nitro-4-aminotoluene	134–135	...	2
4-Nitroaniline	142–143	320	4
3-Nitroaniline	84–85	...	2
2-Nitroaniline	101–102	252–253	4
4-Phenoxyaniline	Oil	274–275	4
3-Phenoxyaniline	Oil	278–279	4

[a] See original reference for structure and behavior on saponification.

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(2) LAPPIN, G. R., unpublished results.
(3) PRICE, E. W., AND J. B. ROBERTS, *J. Am. Chem. Soc.*, 68, 1204 (1946).
(4) RIEGEL, B., R. H. BAKER, *et. al.*, *ibid.*, 68, 1264 (1946).
(5) TARBELL, D. S., *ibid.*, 68, 1277 (1946).

HISTORY OF THE MERCURY FLASK

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SINCE 1927 the common unit for measuring and pricing mercury has been a 76-pound flask. Of the several explanations for this unusual unit, three seem important enough to mention.

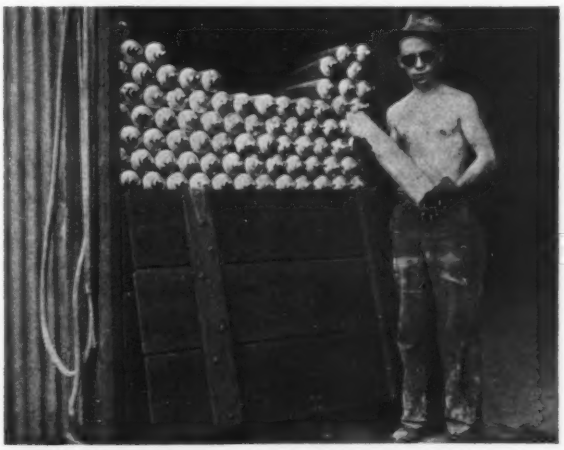
According to information from the U. S. Bureau of Mines, the 76-pound unit was adopted in the United States for the sake of uniformity and agreement with Spanish practice. When the metric system was first used in Spain the content of a standard mercury flask was fixed at 34.5 kilograms, which is equivalent to 76.06 pounds, approximately 76 pounds, one ounce. Since the commercial production of mercury probably originated in Spain it seems reasonable to believe that the 34.5 kilogram unit had its beginning there. Before the adoption of the metric system there were a number of different "pounds" or "libras" in use in different parts of Spain and for different commodities, just as there are different bushels used in the United States today. However, there was a legal apothecary's libra which was equivalent to 0.7606 pound avoirdupois. This unit weighed three-fourths as much as the legal avoirdupois pound, which was 1.0143 U. S. avoirdupois pounds.

The early uses of mercury were largely confined to the medicinal field, and therefore early purchases of this metal may well have been on the basis of the apothecary's pound. Consequently, it seems possible that our 76-pound unit had its origin as 100 Spanish apothecary's pounds, especially in view of the common metric practice of using a multiple of ten, in this case the quintal or hundredweight, to make larger metric units.

Secondly, it has been suggested that the 76-pound weight unit for mercury had its origin as follows: Taking the bushel measure as equivalent to 2150 cubic inches and water at the conventional 250 grains per cubic inch, we find there are 537,500 grains per bushel; at 7000 grains per pound we then get 76.7 pounds as the weight of a bushel of water. At first this rather close approximation seems logical and probable since several of our scientific constants are based on water as the standard. However, it would seem from the facts given above that this explanation is much too modern to be accepted.

The most probable explanation of the origin of the 76-pound unit is that it is related to a Spanish weight, the *arroba*. This Spanish unit is usually equated to 25.36 avoirdupois pounds. Three of these units would, therefore, equal 76.08 pounds or almost exactly 76 pounds, one ounce, the exact contents of a flask of mercury, rather than 76 pounds as it is usually stated.

The flask itself, as may be seen in the illustration, is approximately 18 inches long and 4 inches in diameter. It is made of cast iron and fitted with a $^3/_4$-inch threaded iron plug. The weight of this container, plus the three *arrobas* of mercury, is about all a man can be expected to handle conveniently and regularly. Perhaps it would be more nearly correct to say all that a slave could handle, as the unit was used as early as Roman times when slaves and prisoners of war worked the Almaden and Idrian mines in Spain.



Those persons associated with cinnabar mines and mercury distilleries in California, as far back as they can remember, can recall no other unit ever having been used, and the standard flask in use today contains 76 pounds, one ounce. It is true, however, that from some of the smaller mines the metal is sometimes sent out in sheepskin bags holding 55 pounds of mercury, but this unit does not appear in commerce.

THE GEM-TESTING LABORATORY OF TIFFANY & CO.

A. E. ALEXANDER
Tiffany & Co., Fifth Avenue and 57th Street,
New York, New York

For many years Tiffany & Co. has maintained a gem-testing laboratory for its own use. From time to time new and practical equipment has been added until today this gem-testing laboratory is one of the most complete of its kind. The purpose of it is to insure that all gem stones and genuine pearls purchased by Tiffany & Co. meet with its standard of quality, perfection, and value. The firm does not sell cultured pearls nor synthetic gem stones.

A description of the more important pieces of equipment in this laboratory and the use to which they are put follow.

(1) The binocular microscope, of which the store has several, is unquestionably the most practical instrument, because it can be used to test and examine all gem stones. It affords the distinct advantage of stereoscopic vision disclosing under correct illumination telltale inclusions which are so important in identification. The binocular microscope has no equal for examining diamonds for perfection. The instrument is also used in conjunction with a Leica camera and accessories which can be easily inserted in the eyepiece holder as shown in Figure 1. To facilitate the examination of gems, especially diamonds, an optical glass cell is used. The gems to be examined are placed table and crown facets up in the glass receptacle and covered with a clear liquid of known refractive index. Ordinary mineral oil will do. As many as twenty one-carat diamonds can be examined at one time. Immersing

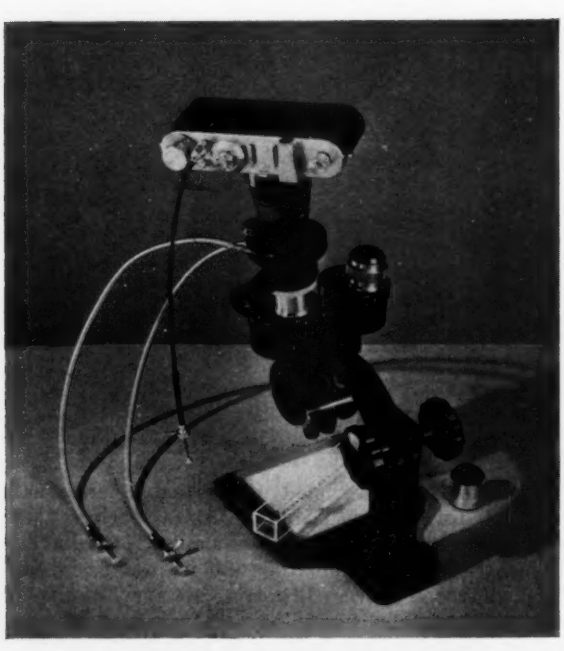
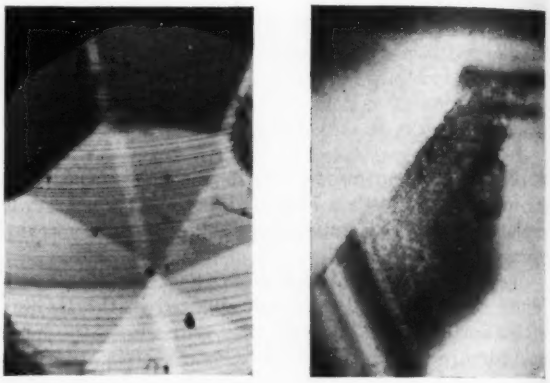


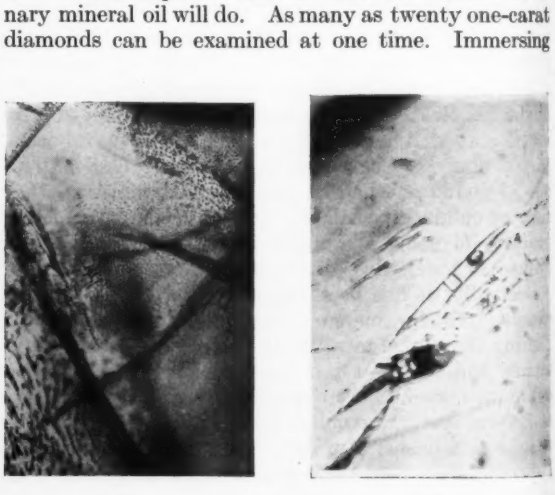
Figure 1. Binocular Microscope and Camera Equipment for Examining and Photographing Gemstones and Pearls



(a) (b)

Figure 2

(a) Curved lines in synthetic ruby are characteristic of this type of stone.
(b) Rutile needles in Burma ruby are proof of genuiness.



(a) (b)

Figure 3

(a) Finger print in synthetic emerald, an inclusion never seen in real emerald.
(b) Three-phase inclusion found only in Colombian emeralds of natural origin.

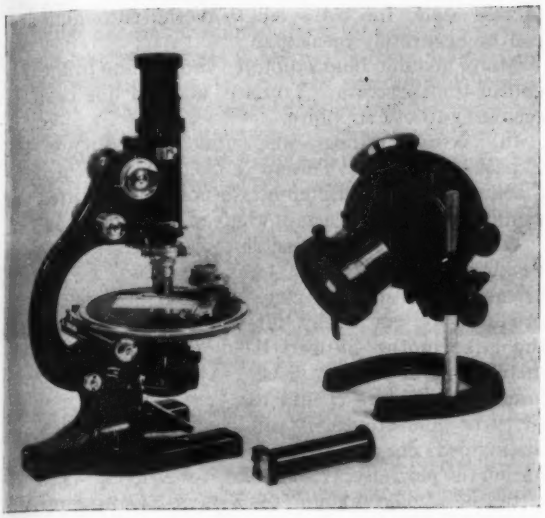


Figure 4. Petrographic Microscope for Examining Powdered Mineral Specimens

the stones in liquid cuts down annoying internal light reflections. The use of such a cell aids in bringing out the fine structure of very minute inclusions of a gem stone, such as those of a ruby, for example, so that they can be satisfactorily photographed. In Figure 2*a* the curved lines of a synthetic ruby are clearly shown. These lines develop during the formation of the boule. A description of the methods used in the manufacture of synthetic corundum can be found in the September, 1946, issue of THIS JOURNAL.

Genuine rubies of Burma origin frequently contain minute needles of rutile which intersect at angles of 60 and 120 degrees, as in Figure 2*b*.

Synthetic emerald may be identified by peculiar veillike or "fingerprint" inclusions of the type shown in Figure 3*a*. These inclusions are never found in genuine emerald. Colombian emeralds, mined near Bogota, are characterized by the presence of "three-phase inclusions." These consist of a gas bubble and a minute, perfectly formed crystal of rock salt (See Figure 3*b*). Inclusions such as these are never developed in synthetic emerald.

(2) When a powder analysis must be made on mineral matter, a petrographic microscope is required equipment. Determination of refractive index, crystal system, optic sign, and birefringence and the performing of certain microchemical tests are possible with the successful operation of this type of microscope (see Figure 4).

Inclusions in gem stones and structures revealed in thin-sectioned pearls can be photographed at high magnification with Leica camera equipment.

A photomicrograph of typical fresh water shell structure (the same as is found in the nucleus of a Japanese cultured pearl) previously thin-sectioned, as taken through Tiffany & Co. petrographic microscope, is shown in Figure 5.

(3) Refractometers are necessary to measure the refractive indices of gem stones. Each mineral or gem species has its own specific index of refraction. For example, genuine red spinel has an index of 1.715, genuine ruby, which is doubly refractive, has indices of 1.765–1.773. Thus, the refractometer is an aid in identifying different gem stones when there is no overlapping or coincidence. Where the indices for a synthetic are the same as for a natural stone as in the case of corundum (ruby and sapphire) other tests must be made. Tiffany & Co. have several types of gem-testing refractometers, as shown in Figure 6.

(4) A polariscope (see Figure 7) is useful in determining whether a gem stone is singly or doubly refracting. This instrument consists of two polaroid plates set with their planes of polarization at right angles to each other and illuminated by a 60 watt lamp. A stone to be tested is inserted between the plates and then slowly rotated 360 degrees. If the stone is singly

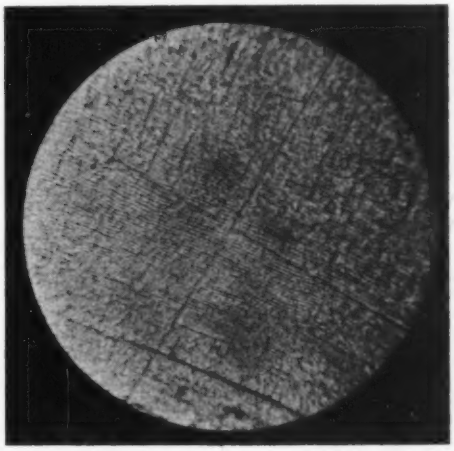


Figure 5. Photomicrograph of Thin-Sectioned Mother-of-Pearl Shell Showing Typical Block-like Structure of the Mineral Aragonite

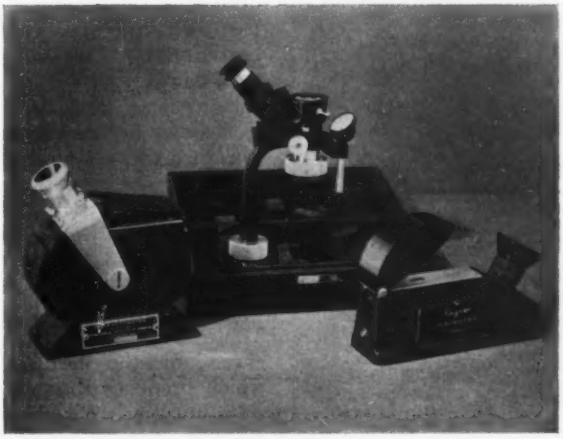


Figure 6. Refractometers for Measuring the Indices of Refraction of Gem Stones

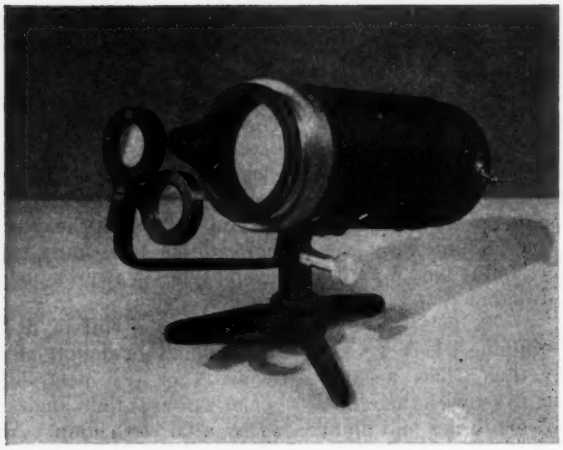
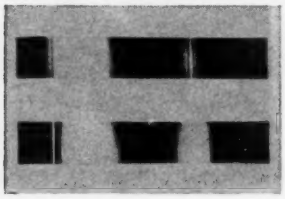


Figure 7. Polariscope Which Permits Separating Singly and Doubly Refracting Stones

refractive it remains dark during rotation. If the stone is doubly refractive it will be found to be alternately light and dark as it is turned in the plane of the polaroid plates every 90 degrees. Furthermore, if a stone is in a state of strain, anomalous double refraction will be seen. This is noticeable in the case of diamond and synthetic spinel.

Figure 8. Absorption Spectroscope for Identifying Absorption Lines in Colored Gemstones



Courtesy of B. W. Anderson

Figure 9. Absorption Spectra of a Genuine Ruby

(5) Each colored gem stone has its own absorption spectral characteristics. To determine the difference in the color spectrum produced by light passing through a stone, measurements can be directly made by the use of a gem absorption spectroscope. Dr. E. J. Gubelin, of Lucerne, Switzerland, has developed such an instrument (see Figure 8). The barrel of this optical apparatus contains a small spectroscope, the base encloses a 6-volt lamp, plus rheostat. If a ruby is placed on the stage and correctly illuminated, several narrow black lines will be seen in the red end of the spectrum (see Figure 9). These are the lines for chromium—the element which produces the red color in ruby. A switch can be turned which brings into view a wavelength scale. By means of this scale, the wave length of the chromium lines can be accurately measured.

Many genuine blue sapphires show a line in the blue end of the spectrum. To date, no such line has been seen in synthetic sapphire.

(6) X-ray radiographic unit (see Figure 10). Especially built for Tiffany & Co., this Westinghouse machine is used to differentiate not only cultured and genuine pearls, but certain kinds of gem stones as well.

With this unit it is possible to X-ray an entire pearl necklace in a matter of 20 to 30 seconds. If the pearls are genuine, a series of concentric growth rings will be produced on an X-ray film. If they are cultured, the large mother-of-pearl bead, coated with a millimeter or so of nacre, will be clearly seen on the radiograph (see Figures 11 and 12). In testing for authenticity of gem stones, it can be used for example to distinguish corundum from rutile or zircon. The former is partially transparent to X-rays, while the latter is wholly opaque to X-rays. Corundum will cast a gray shadow on X-ray films; rutile and zircon, blocking the rays, will develop out white. Diamond being totally transparent to X-rays will be barely visible on photographic film.

Fluorescence in gem stones and pearls can also be obtained with this unit. Japanese cultured pearls,

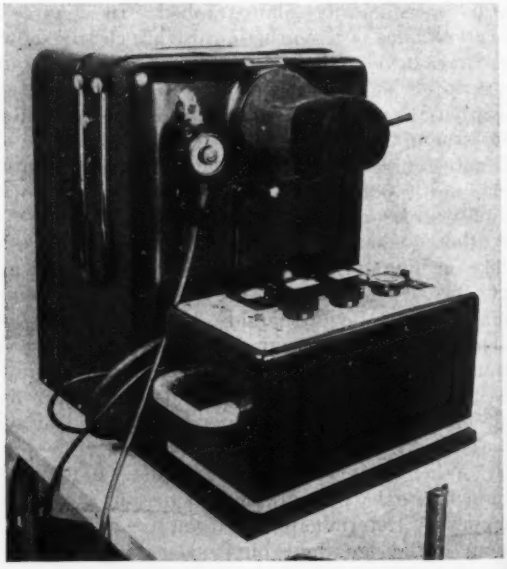


Figure 10. X-Ray Radiographic Unit for Radiographing Pearls and Stones

when charged with X-rays, will in general fluoresce. This is due to the fact that the mother-of-pearl core is derived from fresh-water shell which contains a trace of the element manganese. It is the presence of this element which causes the fluorescence in this type of shell. Genuine pearls from the Persian Gulf contain no manganese, and therefore will not fluoresce. Observing fluorescence in pearl, however, is

only an indication that the pearl or pearls under examination are not genuine salt-water pearls.

Gems may or may not fluoresce. Emeralds of Muzo, Colombia origin, do not; synthetic emeralds have been found to produce a dark maroon-red fluorescence. Again, some stones following X-ray irradiation will phosphoresce, others will not. Synthetic rubies for the most part will reveal a phosphorescence; genuine rubies will not.

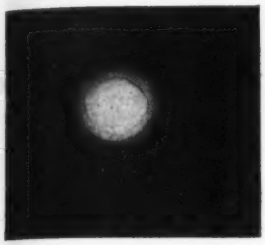


Figure 11. An X-Ray Radiograph of a Genuine Pearl

(7) X-ray diffraction unit. A General Electric X-ray diffraction machine designed specifically for pearl identification is an important part of Tiffany & Co.'s gem-testing equipment (see Figure 13). In order to understand how this unit works a short description of the physical properties

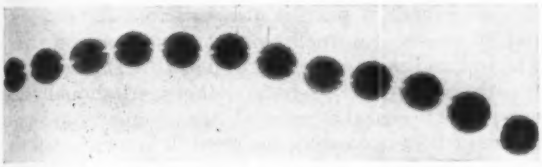


Figure 12.—An X-Ray Radiograph of a String of Cultured Pearls (Note Large Mother-of-Pearl Cores)

of a genuine and a cultured pearl follows. A genuine pearl consists of about 96 per cent calcium carbonate held together by an organic binder known as conchiolin. The carbonate may be in the form of aragonite, calcite, or both. The mineral matter is deposited in a

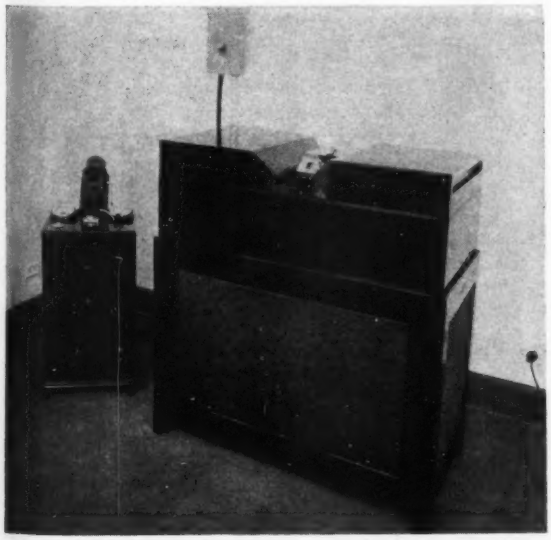
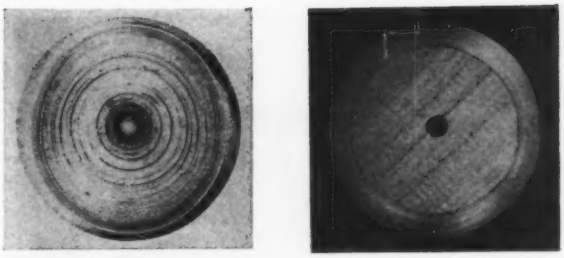


Figure 13. X-Ray Diffraction Unit for Testing Pearls



(a) (b)

Figure 14

(a) Photograph of a thin-sectioned genuine pearl (note typical concentric growth rings).
(b) Photograph of a thin-sectioned Japanese cultured pearl (note large mother-of-pearl core and stratified structure).

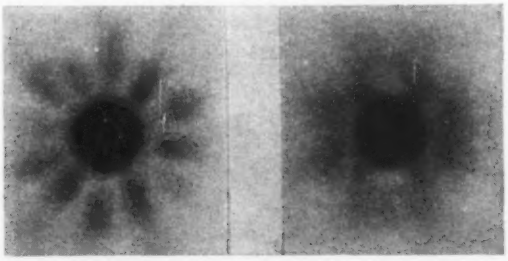
concentric manner best illustrated by the photograph of a thin-sectioned, genuine pearl in Figure 14*a*.

A Japanese cultured pearl, on the other hand, usually consists of a large mother-of-pearl bead which has been derived from fresh-water shell. This bead normally measures from 80 to 90 per cent of the total linear diameter of the pearl. The nacreous coating which the pearl-oyster deposits around the core is approximately one millimeter in thickness. A thin section of a Japanese cultured pearl clearly shows the extent of the mother-of-pearl bead as well as delineating the parallel structure of the mineral matter which characterizes typical fresh-water shell (see Figure 14*b*).

When a narrow beam of X-rays is directed against a genuine pearl, the rays are diffracted or split up inside the pearl. This results in the halo or spoke-like pattern which registers on an X-ray film in the manner shown in Figure 15*a*.

On the other hand, when an X-ray beam is projected against a cultured pearl, a Maltese-cross pattern results as shown in Figure 15*b*. The stratified arrangement of the mother-of-pearl shell is the cause of the "cross" forming instead of a halo or spoke design.

(8) The Endoscope (see Figure 16). This ingenious optical instrument was invented in 1927 by two Frenchmen, Chelowsky and Perrin. While designed



(a) (b)

Figure 15

(a) Typical spoke-like diffraction pattern produced by a genuine pearl.
(b) Maltese-cross diffraction pattern produced by a Japanese cultured pearl.

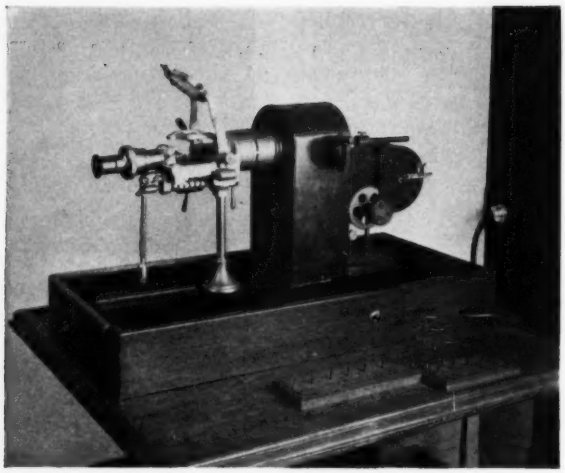


Figure 16. Endoscope Used to Detect Light Flash in Interior of a Genuine Pearl. Cultured Pearls Produce No Flash

for testing drilled pearls only, the instrument is quite effective in separating genuine pearls from those cultured by the Japanese. The endoscope consists of an arc lamp which projects light through an optical system to which, by means of a holder, is attached a hypodermic-like needle. On the end of this needle are two opposed minute platinum mirrors, set at angles approximately 45 degrees to each other. The pearl to be tested is slipped over the needle, brought into line with the converging light of the optical system, and then locked in place. A device permits the pearl to be moved backward and forward over the needle.

If the pearl on the needle is a genuine one, a small, but distinct flash will be seen in the eyepiece as the pearl is moved over the needle. The flash is due to the fact that light reflected from the first mirror of the needle is directed into the interior of the pearl. This light (in the pearl) is then deflected downward at approximately right angles. It is this light which is picked up on the outside mirror to produce the flash as mentioned above. A Japanese cultured pearl, because of its large mother-of-pearl core composed of striated layers, will not produce a flash when tested on this instrument.

(9) The Pearl Compass. Early in 1920, Dr. R. Nacken, a German physicist, constructed what he called a "pearl compass" (see Figure 17). It makes use of the fact that some mineral matter is diamagnetic while other mineral compounds are not. To operate the instrument, a pearl is attached to a slender glass rod by means of a small amount of nonmagnetic wax. The rod and pearl in turn are then attached to a very fine thread. When assembled the pearl should hang exactly between the poles of the magnet. When a magnetic field is created, the pearl, if genuine, will not rotate. However, if the pearl under test is cultured, a slow rotation of the pearl will be observed. Care must be exercised to insure that the pearl under test, if drilled, does not contain a piece of broken drill bit, or, if half-drilled, some pearl cement, which may be itself diamagnetic. A genuine pearl under such circumstances will be found to rotate.

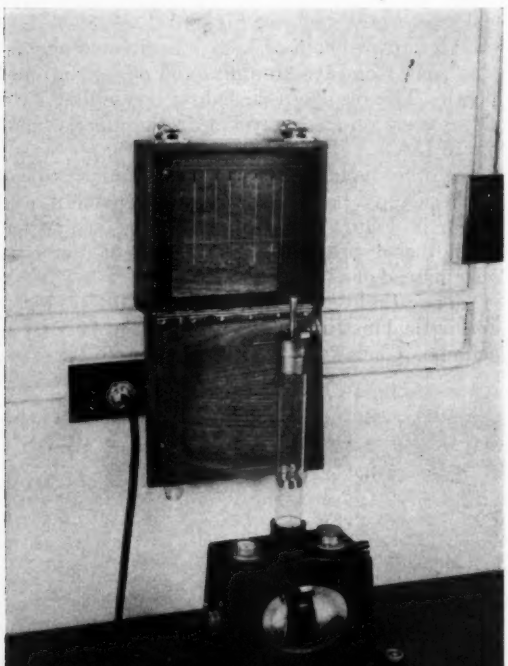


Figure 17. Pearl Compass for Testing Pearls

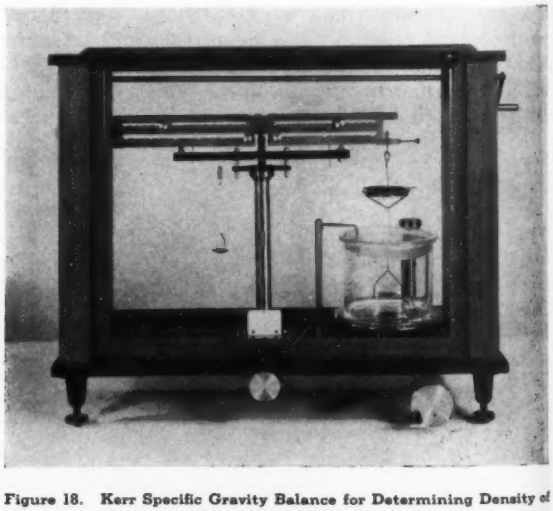


Figure 18. Kerr Specific Gravity Balance for Determining Density of Gemstones and Pearls

(10) Specific gravity balance. Occasionally it is necessary to determine the density of a gem stone. There are several methods and a number of instruments

available to obtain specific gravity. Tiffany & Co. have a balance designed by Dr. Paul Kerr of the Department of Geology, Columbia University (see Figure 18).

Weights and weighing arms are so counterbalanced and calibrated that short cuts are achieved during the gravity determination. In short, the Kerr balance simplifies density determinations. A density balance can prove particularly effective in separating Jadeite from Nephrite. Both minerals are sold in the trade as "Jade" and occasionally they are very similar in appearance. Jadeite, however, is the heavier of the two minerals, having a density of 3.33 as against 3.00 for Nephrite.

A PATENT ASSIGNMENT[1]

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THE importance of patent protection for new or improved processes or products is quite generally recognized by the chemical industries. A progressive organization will nearly always protect the results of its research by means of patents. It therefore follows that students of chemistry who later in their careers may be at least instrumental in and often responsible for advances in applied science should have some knowledge of what a patent is. This is true not only for graduate students but also for undergraduates.

One way to learn something about patents is to study a copy of an actual patent. It is the author's contention that every student who will graduate with a bachelor's degree in chemistry should at least study the structure of one original patent and analyze it from the viewpoint of good practice. For this purpose the following class assignment is proposed.

The student is first required to familiarize himself with *Chemical Abstracts* and the practice of this journal of abstracting patents. *Chemical Abstracts* covers all the important chemical literature of the world, including patents, and publishes this material in abstract form arranged in thirty-one distinct sections. Under each section are contained the abstracts of material peculiar to that section. The individual abstract consists of the patent title, the names of the patentees and assignees (if any), the patent number, the date of issue, followed by a brief synopsis of the subject matter of the patent. From these abstracts the student selects a patent that interests him and secures a copy of the patent by sending twenty-five cents to the Commissioner of Patents, United States Patent Office, Washington 25, D. C.

The following assignment is then made.

Supply the following information concerning a patent in which you are interested and a copy of which you have secured from the United States Patent Office:

1. Patent title.
2. *Chemical Abstracts* reference.
3. Names of the patentees and assignees, if any.
4. Date of application and date of issue of the patent.
5. Patent number and serial number.
6. First state in one sentence, as briefly and inclusively as possible, what the invention is. Then give a short summary of the specification of the invention. Classify the invention as art, machine, manufacture, composition of matter, improvement, or two or more.
7. Clearly show what is useful and new in the invention.
8. Briefly state the claims made and whether in your opinion the claims are sound.

Give reasons for all your statements. The copy of the patent must be submitted with the assignment.

An explanation of most of the concepts covered in this assignment can be found in a pamphlet, "General Information Concerning Patents," a copy of which can be obtained without charge from the Commissioner of Patents. This booklet briefly describes the rights granted by a patent, the qualities necessary for a patentable invention, and the structure of specifications. It will be quite worth while for the student to read this pamphlet before he attempts to carry out the assignment.

Some teachers of chemistry have used assignments similar to that recommended here but they usually confined them to courses in chemical literature. The assignment, however, can easily be adapted to any chemical curriculum. It may be used in any advanced undergraduate course by simply correlating the subject matter of the patent with the general content of the course. Thus in a course in advanced organic chemistry the student might be limited in his selection of the patent to those abstracted in section ten (organic chemistry) of *Chemical Abstracts*.

[1] Based on a paper presented before the Division of Chemical Education at the 118th meeting of the American Chemical Society, Chicago, September, 1950.

STEREOCHEMISTRY OF SIMPLE RING SYSTEMS

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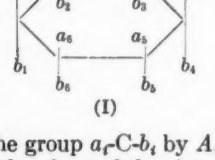
University of Illinois, Urbana, Illinois

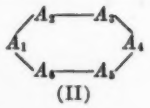
FIESER[1] has introduced a "plus" and "minus" notation for denoting the configurations of asymmetric centers in the dicarboxylic acids derived from sugars. This paper introduces a procedure for examining the stereochemistry of simple carbocyclic and heterocyclic rings, which is essentially a generalization of Fieser's notation.

We will consider, as a starting point, a six-membered carbocyclic system in which the ith carbon atom has attached substituents a_i and b_i, where all a_i and b_i may be the same or different:



(I)

We will denote the group a_i-C-b_i by A_i if a_i lies above and b_i lies below the plane of the ring. The inverse of this group will be denoted by A_i^-. Structure (I) can then be expressed in terms of the A_i as



(II)

To facilitate writing, (II) will be written in a 2 × 3 array, which we will term a *matrix*, symbolized by $|A_i|$:

$$|A_i| \equiv \begin{vmatrix} A_1 A_2 A_3 \\ A_6 A_5 A_4 \end{vmatrix}$$

In setting down a matrix, it is important that the sequence of the A_i as they occur in (I) (or, equivalently, in (II)) and the sense of the sequence (clockwise or counterclockwise) be preserved in the matrix.

The *value* of a matrix, $V|A_i|$, is the actual structure which the matrix represents. The value of $|A_i|$ is structure (I). The plane in which the matrix is written will be called the *matrix plane*, which corresponds to the plane of the ring in (I).

It is apparent that if the elements of a matrix undergo a cyclic permutation, the value of the matrix remains unchanged. Such a cyclic permutation corresponds to a rotation of structure (I) about an axis perpendicular to the plane of the ring. The operation of bringing about any cyclic permutation of the elements in the ring will be denoted by T_i. A rotation of structure (I) of 180° about an axis lying in the plane of the ring flips the molecule over. Analogously, a matrix can be "flipped over" by application of the operation T_0:

$$T_0 \begin{vmatrix} A_1 A_2 A_3 \\ A_6 A_5 A_4 \end{vmatrix} \equiv \begin{vmatrix} A_6^- A_5^- A_4^- \\ A_1^- A_2^- A_3^- \end{vmatrix}$$

The net result of operating with T_0 is that each element is inverted and the sense of the sequence of elements becomes reversed.

There are two operations of reflection. R_i is reflection across any edge of the matrix in a mirror perpendicular to the matrix plane. R_0 is a reflection in a plane parallel to the matrix plane. These operations are illustrated in the following.

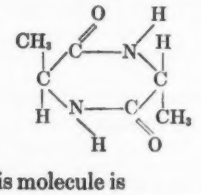
$$R_i \begin{vmatrix} A_1 A_2 A_3 \\ A_6 A_5 A_4 \end{vmatrix} \equiv \begin{vmatrix} A_3 A_2 A_1 \\ A_4 A_5 A_6 \end{vmatrix} \qquad R_0 \begin{vmatrix} A_1 A_2 A_3 \\ A_6 A_5 A_4 \end{vmatrix} \equiv \begin{vmatrix} A_1^- A_2^- A_3^- \\ A_6^- A_5^- A_4^- \end{vmatrix}$$

It is seen that R_i reverses the sense of the sequence of elements leaving the elements themselves uninverted, whereas R_0 inverts each element but leaves the sense of the sequence intact.

If, for two operators P_1 and P_2, $P_1|A_i|$ and $P_2|A_i|$ are identical, then P_1 and P_2 are identical ($P_1 \equiv P_2$). If the values of $P_1|A_i|$ and $P_2|A_i|$ are equal (*i. e.*, $P_1|A_i|$ and $P_2|A_i|$ represent the same structure), then P_1 and P_2 are equal ($P_1 = P_2$). That operation which, when applied to a matrix, leaves its value unchanged is the unity operator, one. In view of the preceding definitions, $T_i = T_0 = 1$. Any operation which forms a mirror image of a matrix will be designated by E. It can be shown that $R_i = R_0 = E$. If, for a given structure, it can be shown that $E = 1$, the structure is either a *meso* form or inactive because of a higher degree of symmetry. By definition, a *meso* form is one which possesses asymmetric centers but is optically inactive because it possesses a plane or center of symmetry.

The compound operator P_2P_1 signifies that P_1 operates on the matrix first and P_2 operates on the result. If $P_1 = P_3$, then $P_2P_1 = P_2P_3$; this holds for the identity relation as well.

Consider the *trans* diketopiperazine of alanine:

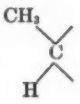


The matrix for this molecule is

$$|A_i| \equiv \begin{vmatrix} A_1 A_2 A_3 \\ A_3 A_2 A_1^- \end{vmatrix}$$

[1] FIESER, L. F., AND M. FIESER, "Organic Chemistry," 1944, p. 268.

where

$A_1 =$  , $A_2 = >C=O$, $A_3 = >N-H$

Then,

$$R_0|A_i| \equiv \begin{vmatrix} A_1^- A_2 A_3^- \\ A_3^- A_2 A_1 \end{vmatrix} = E|A_i|$$

From the steric nature of A_1 and A_3, $A_2^- = A_2$ and $A_3^- = A_3$; thus,

$$E|A_i| = \begin{vmatrix} A_1^- A_2 A_3 \\ A_3 \quad A_2 A_1 \end{vmatrix} \equiv T_i \begin{vmatrix} A_1 A_2 A_3 \\ A_3 A_2 A_1^- \end{vmatrix} \equiv T_i|A_i| = 1|A_i|$$

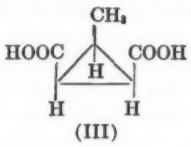
Thus we have shown for this molecule that $E = 1$, and since the molecule possesses centers of asymmetry, it is a *meso* form.

The matrix notation can be extended to rings other than those with six members. An eight-membered ring would be represented by a 2×4 array. Rings with an odd number of atoms can be expressed in a rectangular array by introducing the null element, O, which serves merely to fill in the array. Although the position of O in the matrix is perfectly irrelevant, for the sake of definiteness it will be assigned a position between the first and last elements in the matrix; thus a three- and a five-membered ring would be represented as follows:

$$\begin{vmatrix} A_1 A_2 \\ O \quad A_3 \end{vmatrix} \qquad \begin{vmatrix} A_1 A_2 A_3 \\ O \quad A_5 A_4 \end{vmatrix}$$

It will be further required that the position of O relative to these two elements be maintained under all operations.

A very simple example of the application of this notation to the determination of the number of isomers of a given compound follows. We will consider 1-methylcyclopropane-2,3-dicarboxylic acid (III):



(III)

The matrix for this compound is

$$|A_i| \equiv \begin{vmatrix} A_1 A_2 \\ O \quad A_1 \end{vmatrix}$$

There are two possible signs for each element, hence $2^3 = 8$ possible matrices which may represent different compounds. Out of these eight matrices there will be four in which the signs of the A_1's are alike:

$$\begin{vmatrix} A_1 A_2 \\ O \quad A_1 \end{vmatrix} \quad \begin{vmatrix} A_1 A_2^- \\ O \quad A_1^- \end{vmatrix} \quad \begin{vmatrix} A_1^- A_2 \\ O \quad A_1^- \end{vmatrix} \quad \begin{vmatrix} A_1^- A_2^- \\ O \quad A_1^- \end{vmatrix}$$
(IV) (V) (VI) (VII)

Since R_0 (IV) $\equiv$ (VII) $= E$ (IV) and T_0 (VII) $\equiv \begin{vmatrix} O & A_1 \\ A_1 & A_2 \end{vmatrix} \equiv T_i$ (IV), it follows that (IV) = (VII) (*i. e.*, (IV) and (VII) have the same value) and that E (IV) = (IV) (*i. e.*, (IV) has the same value as its mirror image). For (IV) it has thus been established that $E = 1$, and since the A_1's in (IV) are asymmetric centers, it follows that (IV) represents a *meso* form, and that (VII) represents this same compound. Similarly it can be shown that (V) and (VI) represent the same compound which is a *meso* form. Inspection shows that there is no way of interconverting (IV) and (V) (or (VI) and (VII)) due to the nature of the difference in signs. Thus there are two *meso* forms corresponding to the given compound. The four matrices in which the two A_1's are different are:

$$\begin{vmatrix} A_1 A_2 \\ O \quad A_1^- \end{vmatrix} \quad \begin{vmatrix} A_1 A_2^- \\ O \quad A_1^- \end{vmatrix} \quad \begin{vmatrix} A_1^- A_2 \\ O \quad A_1 \end{vmatrix} \quad \begin{vmatrix} A_1^- A_2^- \\ O \quad A_1 \end{vmatrix}$$
(VIII) (IX) (X) (XI)

Application of the operation R_0 shows that (VIII) and (XI), and (IX) and (X) are mirror images. Application of the operation $T_i T_0$ shows that (VIII) and (IX), and hence (X) and (XI) have the same value. Since the senses of the sequence of elements in (VIII) and (X) are opposed, these two are not interconvertible, and one *d,l* pair is possible for the given compound.

ACKNOWLEDGMENT

The author wishes to express his appreciation for the mathematical assistance rendered by James P. Rizzo, Stanton Preparatory Academy, Cornwall, New York.

INSTITUTE FOR THE TEACHING OF CHEMISTRY

St. Louis University has announced the establishment of the Institute for the Teaching of Chemistry. The object of the Institute is to provide broad as well as intensive training for teachers of elementary chemistry in the secondary schools and in Junior Colleges. The program is primarily designed for the in-service training of teachers already in the field, although many of the courses may be credited toward the Master's degree in the Teaching of Chemistry.

The six-week program of the Institute begins June 19 and includes the following: (*a*) courses in the several fields of chemistry and physics, dealing with subject matter of direct value to the teacher of elementary chemistry; (*b*) seminars in special topics of chemistry; (*c*) seminars in teaching problems of elementary chemistry, including a course in lecture-demonstrations; and (*d*) special lectures and informal conferences.

For further information write to Dr. Theodore A. Ashford, Director of the Summer Intitute, Department of Chemistry, St. Louis University, St. Louis, Missouri.

JOHANN NEPOMUK FUCHS

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(Translated by Ralph E. Oesper)

THE term "isomorphism," coined by Berzelius, denotes the fact that similar compounds of various elements may have like crystal forms. This phenomenon was discovered early in 1819 by Eilhard Mitscherlich in Berlin, first with the salts of phosphoric and arsenic acid, then with sulfates of bivalent metals.

Isomorphous elements can replace each other in their respective corresponding compounds in indefinite proportions and form homogeneous mixed crystals, in which they occur together in nonstoichiometric ratios. This fact had been known several years prior to Mitscherlich's first observations.

Since 1807, the chair of chemistry and mineralogy at the university, which was founded at Ingolstadt in 1472 and moved to Landshut in 1800, had been occupied by Johann Nepomuk Fuchs. As a pupil of Klaproth and the Paris school, and in conformity with the demands of this stage of chemical development, he had occupied himself at first with the analyses of minerals in order to find out whether their compositions were in accord with the laws of definite and multiple proportions. The atomic theory and the chemical system of symbolic notation were just taking form. It was still the fashion to follow Lavoisier and regard all chemical compounds and minerals as simple or compound oxides. About this time, Berzelius determined "the weight of the elementary quantity compared with that of oxygen gas," whose quantity or volume he represented by 0 and arbitrarily set equal to 100, *i. e.*, he determined the atomic weights of the elements referred to O = 100. The procedure was to determine by quantitative methods the percentage content of the simple oxides in a natural or artificial compound and then to calculate the volumes of oxygen which must have been contained in these oxides. In chemical compounds these volumes are in the ratio of small whole numbers; for example, in sulfates 1:3, in nitrates or chlorates 1:5, and so forth.

Die Wissenschaft ist der Leitstern der Praxis
und dient verirrt sich ohne sie leicht in dürftern und
unbegrenzten Felde der Möglichkeit.

Johann Nepomuk Fuchs in 1844

"Science is the lodestar of practice, without it the latter easily loses itself in the murky and infinite ranks of possibility."

Fuchs conceived the plan of analyzing as accurately as possible whole groups of similar minerals, *e. g.*, garnets from various regions. Beginning in 1813, he and his friend Adolph Ferdinand Gehlen, professor of chemistry at the Bavarian Academy of Sciences in near-by Munich, studied zeolites. This name (Gr. *zeein* = to boil, and *lithos* = a stone) had been coined in 1756 by the Swedish mineralogist Axel von Cronstedt (1722–65) as a collective term for hydrous, acid-decomposable silicates, which foam under the blowpipe because of the evolution of steam and then melt to a glass. Later, these silicates were classified, according to their appearance, as fibrous-, foliated-, and needle-zeolites: mesotype or natrolite, stilbite, analcite, etc. The two friends soon discovered that several varieties of mesotype had the same composition as Klaproth's natrolite, whereas others with the same oxygen ratios contained lime in place of soda. Since these warped under the blowpipe they called them scolecites (Gr. *scolio's* = curved). However, they subsequently found a scolecite which contained considerable proportions of soda along with lime. In 1815 Gehlen (b. 1775) died prematurely as a result of arsine poisoning. Fuchs carried on alone and published the findings in Schweigger's *Journal für Chemie und Physik* [**18**, 1 (1916)]. Since the lime-soda-bearing zeolite was intermediate between natrolite and scolecite with respect to composition and properties, he proposed that it be named mesolite (Gr. *mesos* = in the middle). In modern terms, all three minerals form an isomorphous mixed series.

In addition to this study of zeolites Fuchs analyzed a new mineral which had recently (1814) been found in

South Tyrol. He suggested that this lime-alumina silicate be called gehlenite, in honor of his late friend and co-worker. In his report[1] he stated that the results of the best analyses "agree, as may be seen from the accompanying quantities of oxygen, fairly well with the theory of definite chemical proportions. . . . However the situation is peculiar with respect to the lime and iron oxide; neither one or the other taken by itself fits into the series of ratios, but if they are taken together, a quantity of oxygen is obtained which is almost exactly four times that of the oxygen of water, and almost equal to the oxygen of alumina. Nevertheless, I do not regard the iron oxide as an essential constituent of this variety, but merely as a *vicariating constituent*, if I may be permitted to employ this expression to designate a substitute for almost as much lime, which in the absence of the iron oxide must be present to make up the full complement in order to enter into the proper ratio with the other constitutents; and I believe that in the future varieties will be discovered which will contain much less or no iron oxide at all, but instead a greater quantity of lime. The whitish crystals, which merely turn a light yellowish grey in the fire, and which appear to be somewhat more refractory, seem to constitute the transition to this.

"The results of several analyses of mineral compounds must be looked at from this viewpoint if it is desired, on one hand, to bring them into accord with the theory of proportions, and on the other if it is desired to avoid having the varieties unnecessarily dispersed too much . . . . Aluminum sulfate forms an alum with ammonium as well as with potash, or with both of these alkalies at the same time; would it be really expedient to look on these three different combinations, which do not differ at all in their physical characteristics, as three different species of salt? The ammonium can replace the potash here altogether or in part, and vice versa."

Fuchs also cited the case of feldspar, in which the potash can be replaced by soda. Therefore, Fuchs was the first (1815) to state that in minerals, and also in artificially prepared chemical compounds, certain constituents can substitute for one another, *i. e.*, vicariate, without any noticeable change in their crystal form. This new fact was not the result of a chance observation but rather was derived from wide experience with mineral analyses.

In the course of his studies of zeolites, Fuchs also investigated a mineral from Amberg, which had been designated as a fibrous zeolite. He immediately recognized that the specimen was not a zeolite but that as well as alumina and water it contained phosphoric acid (22 per cent). Since no previous case of such mineralized phosphate had been reported Fuchs believed that he had discovered a new mineral. However, in 1818 he realized that this material was identical with wavellite, which had been studied by Humphry Davy (1805) and Klaproth (1810), but that its high content of phosphoric acid had completely escaped both of these eminent chemists. Fuchs also found (1818) that lazulite, a blue mineral from the Salzburg region and Styria, which had hitherto been considered a silicate, likewise is a phosphate containing almost 42 per cent phosphoric acid, which Klaproth had missed entirely. Another rare mineral, hitherto considered to be topaz, was recognized by Fuchs as a fluoriferous magnesium phosphate; he named it wagnerite. In 1834 he discovered, at Bodenmais in the Bavarian forest, a lithium-bearing iron-manganese phosphate which he named triphyline, in allusion to its containing three bases.

Birthplace of J. N. Fuchs in Mattenzell in the Bavarian Forest

Johann Nepomuk Fuchs was born on May 15, 1774, in the Bavarian village community of Mattenzell. His parents were poor peasants, who lived in a miserable hut where he was born. His first schooling was obtained in the nearby monastery and he proved so superior among his fellow pupils that he was destined for the clergy. Consequently, in his 17th year he was sent to the episcopal Gymnasium at Regensburg. After the usual humanistic preparatory course, at the age of 20 he was enrolled in the Bavarian university at Ingolstadt where he took up philosophical studies. However, he felt no inner urge for the clerical profession and found patrons who made it possible for him to study medicine at the University of Vienna. He was attracted to science more than to medicine, and chemistry interested him particularly. Natural history, especially botany and chemistry, had been taught at Vienna since 1768 by Nicolaus Joseph Freiherr von Jacquin, whose son Joseph Franz had assisted him since 1791 as adjunct and from 1797 as professor. Under the influence of these men, who at that time were the chief representatives of Austrian sciences, Fuchs turned more and more to chemistry and mineralogy. However, he completed

[1] Schweigger's *Journal für Chemie and Physik*, 15, 377 (1815).

Adolph Ferdinand Gehlen, 1775–1815

the medical course and took his Doctor of Medicine degree at the then Palatinate-Bavarian University of Heidelberg. He passed his state medical examination at Munich but then decided not to enter the practice of medicine because he wished to devote himself exclusively to chemistry and mineralogy. The need for chemists was urgent at the time and it was not difficult to secure a government grant to further his plans. He was thus able to visit the centers where his chosen fields were being practiced in the best manner: Freiberg in Saxony, where lived the great "teacher of mineralogy for all Europe," Abraham Gottlob Werner; Berlin, where the German past-master of chemistry Martin Heinrich Klaproth was active; and finally Paris, where were gathered the leading French chemists Guyton de Morveau, Fourcroy, Berthollet, and Vauquelin, as well as the founder of crystallography, René Just Haüy. After Fuchs had also acquired practical experience on several mineralogical and geological expeditions, he returned to Munich and passed an examination before a committee of the Bavarian Academy of Sciences, with the result that in the fall of 1805 he was installed at the University of Landshut as teacher of chemistry and mineralogy. Georg Augustin Bertele had been active there since 1794 as professor of chemistry, general sciences, botany, pharmacy, and materia medica. Fuchs now held lectures in chemistry and mineralogy in Bertele's fairly well-equipped laboratory. From 1807 on he held the rank of professor. He assembled a collection of chemical preparations, arranged and enlarged the mineral collection, and improved various pieces of laboratory equipment. From 1820 on he also gave a course in analytical chemical exercises, which was open to a maximum of eight students who must have previously shown their merit. In addition, he carried on the mineral studies described above.

In 1810 Fuchs married the daughter of one of the most prominent citizens of Landshut, the owner of the inn at which the university professors held their evening social gatherings. Fuchs thus became well-to-do and acquired a house of his own. He was of good stature, but made no pleasant impression at first contact. It was necessary to become accustomed to his characteristic inquiring look, which was combined with nearsightedness and an ironical smile. As a result of the privations and exertions during his boyhood and student years his lungs were weak when he came to Landsbut; at times he brought up blood, so that his friends were fearful about his life. Although this condition improved, Fuchs was always sickly and had to be careful of himself. Nevertheless, by following a strict regimen, conserving his forces, and avoiding all detrimental influences, he reached the ripe age of almost 82 and, within the limits set by his constitution, was indefatigable to the very end of his life. Because of his nearsightedness he bent low over his writing desk, his arms drawn close to his body, and he often had in front of him a little bowl containing chloride of lime and a rod for stirring it up. His lectures, interspersed with frequent coughing, were not attractive, but they were well prepared and sincere; his experiments seldom failed. Accustomed to privations from his youth, he held economy to be one of the principal virtues, and he practiced it in the laboratory with respect to apparatus and chemicals. The loss of a vessel, through the carelessness of a coworker, could put him into a state of great agitation. He was painfully exact and reliable in the fulfillment of the duties he assumed. A pious, strict Catholic, Fuchs was lovable and always ready to help his fellow man.

Joseph Franz Freiherr von Jacquin, 1766–1839

Although not fond of scientific disputes, he was perfectly capable of putting up a forceful, stubborn, fact-filled defense when he was drawn into such debates.

His poor health hampered his teaching, and his chief merit was as an investigator. Here, as in his daily life, he was extremely conscientious and trustworthy. The results of his researches were highly regarded in professional circles because they invariably proved to be correct.

In the fall of 1823, Fuchs was called to the Bavarian capital as curator of the national mineralogic collections and to become a member of the Academy of Sciences. The president of this learned body was the well-known philosopher Friedrich Wilhelm Schelling, and its chemist was Heinrich August Vogel, who had succeeded Gehlen in this post in 1816. Fuchs inaugurated his activities in Munich with a public lecture "The Mutual Influence of Chemistry and Mineralogy," in which he pointed out that chemistry was indispensable to the founding and progress of mineralogy, since a classification of minerals is only possible on the basis of their chemical constitution and crystal form.

The University of Landshut was moved to Munich in 1826, and Fuchs was again installed as ordinary professor of mineralogy, while Vogel was appointed to the corresponding chair of chemistry. Fuchs held his lectures in the Academy headquarters on Neuhauserstrasse, where the collections were housed. The building also contained a small chemical laboratory in which he continued his chemical investigations, with the sole assistance of a hireling whose pay was 50 kreutzers (25 cents) per day. In these modest circumstances Fuchs carried out the significant researches which will now be described.

Beginning in 1821, Fuchs turned his attention to the origin of china clay, *i. e.*, kaolin. He showed that the clay from Obernzell near Passau was not a product of the weathering of the difficultly attackable feldspar, as was generally assumed, but that it came instead from a much more readily decomposable lime-soda-alumina silicate, which he named porcelain spar (porcelainite). Under the action of water and carbon dioxide this clay had lost all of its soda, almost all of its lime, and a third of its silica, and had thus been converted into kaolin.

In the course of this study Fuchs also took up the synthesis of silicates through the mutual action of alkali silicate and alkali aluminate solutions. He found that the resulting precipitates contained not only alumina and silica but also alkali, and that the latter is replaced by lime when the precipitates are treated with lime water. Consequently, he had observed the base-exchange of zeolites as well as the artificial production of natrolite, scolecite, and other zeolites. In this way he became familiar with the chemistry of silicic acid and arrived at one of his most important discoveries, namely water glass.

At that time only two artificial compounds of silicic acid with the alkalies were known. The first (insoluble) was ordinary glass, which, with no regard to its lime content, was thought to be an alkali silicate containing excess silica. The second was the so-called silica liquor, which was prepared by fusing silica with excess alkali and then dissolving the melt in water, or allowing it to deliquesce in moist air. As early as 1818, Fuchs had prepared a new product from finely divided silica, which he obtained by boiling silica liquor with ammonium chloride solution. Concentrated caustic soda or potash was then saturated with this silica powder and the solution evaporated to a thick syrup. On cooling, it congealed to a rigid, translucent glasslike mass which was stable in the air; when powdered it dissolved in water. Fuchs called his product *water glass*. He worked out a method for preparing it on a commercial scale and recommended it as a fireproofing agent for scenery, as an artist's primer, adhesive, lute, etc. As late as 1855, *i. e.*, just one year before his death, he issued, as a sort of testament of his lengthy experience, a collected account of the "Preparation, Properties and Practical Application of Water Glass, including Stereochromy." He coined the latter term (Gr. *stereos* = solid, *chroma* = color) to designate a procedure in which the artist uses water glass as a binding agent for the colors and the background, a method which he had tested in conjunction with well-known Munich painters. It was claimed that this process yielded more durable murals than the old traditional frescoing.

The analytical investigation of silicates and the invention of water glass, together with its technical utilization, led Fuchs into the study of building materials, such as limestone, clay, marl, and particularly the binding agents prepared from these, namely, lime, mortar, and hydraulic cement. He instituted experiments on the burning of limestone and the quenching of lime, the behavior of lime cream toward the ingredients of mortar, especially toward the various kinds of silica and silicates. Therefore he was well equipped to offer a ready and excellent answer to the prize question submitted in 1830 by the Society of Sciences at Haarlem: "Quels sont les caractères, auxquels on reconnaîtra les ciments, qui s'endurcissent sous l'eau? Quels sont les

Neuhauserstrasse in Munich about 1830—the Academy building to the left

principes constituans et quelle est la combinaison chimique, qui s'opère pendant leur solidification?" In his "Über die Eigenschaften, Bestandteile und chemische Verbindung der hydraulischen Mörtel," which was awarded a gold medal, Fuchs stated for the first time the scientific foundations of the requirements which are necessary for the production of a good hydraulic cement, and he proved that its hardening under water is due essentially to a chemical union of opened-up silica with lime, a reaction which proceeds gradually under the influence of the water.

Abraham Gottlob Werner, 1750–1817

During the course of his many studies of mineral natural products Fuchs made a number of remarkable observations. As early as 1809 he distilled petroleum from St. Quirin on the Tegernsee in the Bavarian Lower Alps, and in addition to naphtha obtained a fatlike, unsaponifiable, colorless, odorless, and tasteless substance which was insoluble in water and soluble in alcohol or ether. This material was later (1830) found in wood tar by Karl von Reichenbach at Blansko in Moravia, and again in 1835 by J. B. Dumas in coal tar. Reichenbach was the first to recognize the technical and economic importance of this product, which was subsequently named *paraffine*. In 1822, Fuchs visited the salt works at Hall, near Innsbruck, and found iodine in the mother liquor. He also found this element in various Bavarian mineral waters, and likewise bromine and lithium. He discovered that rose quartz from Rabenstein near Zwiesel, Bavaria contains about one per cent of titanium dioxide and attributed the color of the stone to this foreign constituent.

Through his observations on silica and its natural compounds, Fuchs in 1833 came to the realization that not all of the so-called dense or compact substances, in which no crystalline structure could be discerned, were to be regarded as aggregates of crystals which were so small as to elude perception. Instead, and contrary to this general belief, there are also certain dense substances which really have no trace of crystallization and which accordingly must be designated as being without form or *amorphous*. He contrasted the crystalline condition of solids, *crystallism*, with *amorphism*, and showed that one and the same chemical substance may have different chemical and physical characteristics depending on whether it is in one or the other condition. Pure opal is essentially nothing other than formless, amorphous, and compact silica, whereas quartz (rock crystal) represents definitely shaped silica. Opal is comparable to liquids in that it is a continuum with the same cohesion in all directions. Like liquids and all amorphous materials it exhibits only single refraction. Opal differs from quartz not only in physical characteristics but also in chemical behavior. For example, when ground to a powder with hydrated lime, opal combines with the latter and sets under water; it dissolves in cold dilute caustic solutions, a behavior not shown by even the finest quartz powder. Chalcedony and flint are mixtures of opal with crystalline silica. "Accordingly we must distinguish two states of solidity, that of the shaped and that of the shapeless, and we may not regard solidification and crystallization as being identical." Many materials are known only in the shaped state, some only in the shapeless condition; many can be put into both states. Amorphous materials, which Fuchs also designated as rigid liquids, may arise by dry methods through vitrification or by wet methods through coagulation. Opal is produced by coagulation; ordinary glass from a melt. Under certain conditions glass may crystallize, whereby it becomes cloudy because of the separation of innumerable tiny crystals—a process known as devitrification. Amorphous antimony sulfide (kermesite) can be obtained from crystalline antimony trisulfide (antimonite) by keeping the latter in the fused state for a long time and then rapidly cooling the melt. The black mercuric sulfide, prepared by either a wet or dry method, can be converted into red cinnabar by sublimation and crystallization, and the latter can be changed into the black sulfide again by heating and rapid cooling.

As examples of vitrified minerals, Fuchs cited not only obsidian, pumice, pitchstone, perlite, but also leucite and gadolinite. Coagulated minerals were exemplified by allophane, psilomelane, pitchblende, etc.; organic coagula by the resins, gums, hard coals, animal glues, and so forth. The glowing displayed by many amorphous materials when they are heated to a certain temperature was attributed by Fuchs to "the awakening of the force of crystallization."

He regarded "the infinite face, the sphere, the mother form of the corporeal world" as the elementary building stone of amorphous materials. He pointed out that mechanical forces, such as pulverization, can never convert a crystalline substance into an amorphous one,

i. e., diamond into lampblack; this change can be accomplished only by arousing the internal forces by chemical means. He used the term *deformation* for the process which is the contrary of crystallization, and he was convinced (1833) that every chemical synthesis must be preceded by a deformation, that every material which combines with another must pass through a state of shapelessness, that it must sacrifice its individuality to the chemical product. "Every inorganic compound likewise must put off its form, if it wishes to enter the organic realm and be assimilated in an organic body. Crystallization and life are absolutely incompatible with each other, and as soon as any substance in an organic body begins to take on a crystalline shape, at that instant it falls into the inorganic realm. The crystal is, so to say, the landmark between the organic and the inorganic realms." Fuchs used this occasion to point out how important easily attackable silica is to plant nutrition.

In 1837, and hence several years before the appearance of Liebig's "Die organische Chemie in ihrer Anwendung auf Agricultur und Physiologie" (1840), Fuchs set forth in a lecture his views on the development of the terrestrial sphere. He expressed his conviction that the carbon of fossil coals and resins and that of the entire animate nature had been derived from the atmosphere and that the oxygen of the air had thereby originated at the same time.

During his many years of experience with mineral analysis Fuchs devised new analytical and preparative procedures, and some of these are still in use. At that time the simple ammonium molybdate test for phosphoric acid was not yet known, and occasionally even eminent chemists overlooked the presence of this acid or its salts. Fuchs worked out a method of detecting phosphate by means of the blowpipe. He found that

Heinrich August Vogel, 1778–1867

René Just Haüy, 1743–1822

phosphates, particularly after being moistened with sulfuric acid, impart a bluish green color to the blowpipe flame. He used alkali silicate solution to separate phosphoric acid from alumina. This reagent precipitates the latter quantitatively as alkali aluminum silicate (artificial zeolite) from a solution containing both phosphate and aluminate, the phosphate remaining in solution. In 1831 he proposed the use of barium or calcium carbonate for the separation of tervalent from bivalent iron and other bivalent metals such as manganese, zinc, nickel, and cobalt. This precipitation procedure is still widely used. In 1839 he showed that tervalent iron may be quantitatively determined in the presence of phosphoric acid in hydrochloric acid solution by means of the loss of weight suffered by a strip of pure copper foil used to reduce the ferric solution. If titanic acid is present it is revealed through its reduction to violet titanous chloride.

In 1832 Fuchs prepared tin sesquioxyhydrate by boiling stannous chloride solution with freshly precipitated ferric hydroxide; the hydrochloric acid solution of the product yielded purple of Cassius on reaction with gold chloride solution. The recipe using a mixture of stannous chloride and ferric chloride solution for the preparation of this gold purple goes back to Fuchs, who also expressed the opinion that the product contains amorphous gold.

All of his accomplishments demonstrated that Fuchs was a thoroughly independent investigator, who not only accumulated factual material but also recognized relationships and opened new paths along which others could make still further progress. His chemical studies of zeolites and mineral phosphates, in conjunction with the discovery of vicariation, fundamentally advanced

the systematization of minerals on a chemical basis. He provided the first scientific foundations for the technology of the silicates, hydraulic cement, artificial zeolites, and glass. His theory of amorphism pointed the way to research in the vast region of the noncrystalline materials, from which there has since developed the chemistry of colloids.

In Landshut as in Munich, Fuchs acted as adviser to industry and agriculture; he was consultant to dyers, paper and tobacco manufacturers, to starch makers and brewers. He introduced the manufacture of sugar from beets into Bavaria.

His services as teacher, scientist, and consultant received proper recognition from the state authorities. In 1833 he was appointed chemist in the Upper Medical Council; in 1835 he was named Adviser to the Board of Mines and Salt Works, with retention of his other offices; in 1849 he was raised to the rank of nobleman; and in 1853 he was given the title Privy Councillor. His colleague Schafhäutl, in 1842, honored him by naming a chromiferous muscovite from the Ziller valley, fuchsite.

Johann Nepomuk Fuchs died at Munich on March 5, 1856, of general exhaustion of his vital forces. His "Gesammelten Schriften" together with a biography was issued in 1856 by his student and last assistant at Landshut, Cajetan Georg Kaiser, who later was professor of technology at the University of Munich.

REDUCTION OF BENZOPHENONE BY ALUMINUM ISOPROPOXIDE

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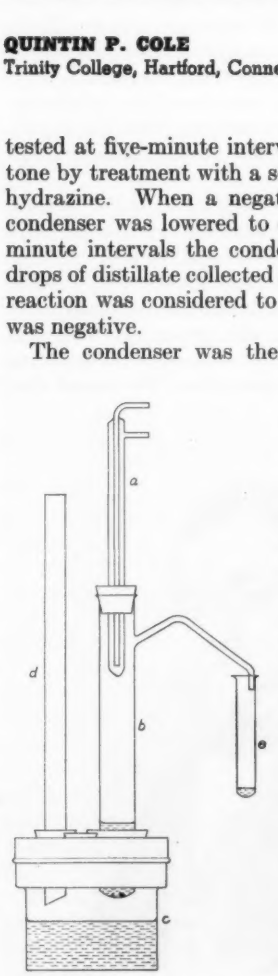
The Meerwein-Ponndorf-Verley reduction of benzophenone by aluminum isopropoxide and isopropanol is reported[1] to give a nearly quantitative yield of benzohydrol when a reasonably efficient fractional distillation assembly is used to remove the acetone formed and the quantity of benzophenone reduced is 0.1 mol.

$$C_6H_5COC_6H_5 + CH_3CHOHCH_3 \xrightarrow{Al(OC_2H_7)_3} C_6H_5CHOHC_6H_5 + CH_3COCH_3$$

In the present work the student use of this procedure on a much smaller scale (less than 0.01 mol) was found to be practical, employing the simple apparatus pictured in the figure. The finger condenser *a*, the 25 ml. distillation tube *b*, and the water bath *c* were standard pieces of apparatus commercially available.[2] The air condenser *d* was a piece of 12 mm. glass tubing and the receiver *e* was a small test tube. The students were supplied with a 1-*M* solution of aluminum isopropoxide in isopropanol prepared by the procedure described by Adams and used without purification. The tendency of the aluminum isopropoxide to deposit as crystals from this solution made it advisable to arrange for use of the reagent within a few days of its preparation.

Ten milliliters of the aluminum isopropoxide solution, 1.5 g. (0.008 mol) of benzophenone, and some boiling chips were placed in the distillation tube. The mixture was heated on the water bath and the rate of distillation adjusted to one or two drops per minute by raising and lowering the condenser. After thirty minutes of continuous distillation the distillate was tested at five-minute intervals for the presence of acetone by treatment with a solution of 2,4-dinitrophenylhydrazine. When a negative test was obtained the condenser was lowered to effect total reflux. At five-minute intervals the condenser was raised and a few drops of distillate collected and tested for acetone. The reaction was considered to be complete when this test was negative.

The condenser was then raised and most of the isopropanol distilled off. Twenty milliliters of 2-*N* hydrochloric acid was stirred into the residue and the crystalline benzohydrol filtered off, washed with dilute acid and water, and dried in the air. It was purified by crystallization from benzene-petroleum ether. The average student yield of purified product was 75 per cent of the theoretical.



Reduction Apparatus

It was found that continuous distillation for about one hour was necessary to obtain the first negative test. Hence, testing of the distillate may properly be deferred that long. A total heating period of 1¼ to 2½ hours was required for complete reaction.

[1] ADAMS, R., "Organic Reactions," John Wiley & Sons, Inc., New York, 1944, Vol. II, pp. 198–203.

[2] Wilkens-Anderson Company, Chicago, Illinois.

SOME VIEWS ON UNDERGRADUATE RESEARCH

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RESEARCH implies original thought and independent investigation and generally can be realized only after certain fundamental techniques and facts have been mastered. The term undergraduate research by necessity carries the same connotation, and we do not believe it should.[2] Undergraduate research should really be preparation for research. The student in these years should be encouraged to be research minded, to pick up the tools and techniques that will help him answer "how" and "why." A thorough undergraduate course is in this sense undergraduate research.

Today the training necessary for independent research is becoming more and more complex. The recent recommendation of the American Chemical Society setting forth the minimum requirements for the bachelor's degree in chemistry illustrates this point. This needed recommendation really telescopes five years of college work in chemistry into the four-year program. The elective hours of a major in chemistry are in effect nonexistent, and perhaps it is just as well. However, if the undergraduate student is to be trained in research technique it would appear that some of his fundamental training in basic course work would have to be sacrificed. This sacrifice in our opinion cannot be justified.

The time a student has available during a school week for additional work is necessarily limited in an undergraduate course. Consecutive long periods of time are necessary for productive research work of any type. We have had limited experience in directing undergraduate research among students who rated in the upper 10 per cent of their class. In attempting to arrange a program for them, some would enthusiastically report that they had two free hours on Monday, three hours on Thursday, and one hour on Friday available for the project. The instructor directing the project found himself doing the undergraduate research. Time and skills in our opinion are wasted on such a project. We believe the ambitious, professionally minded, undergraduate student can profit more from additional and more thorough course and laboratory work if time in an admittedly crowded program permits. The possibility of the better students in the junior and senior classes being assigned problems that would have them repeat some of the current research projects in the laboratory must be considered as a means for encouraging the research attitude in the undergraduate chemist. This sounds novel and refreshing until one realizes that modern texts and laboratory manuals are the results of the brilliant researches and proved work of well-seasoned investigators of a few years back. In this sense again, following additional course work is undergraduate research. We believe the serious undergraduate student can be inspired toward the research attitude, receive the same thrill of accomplishment, have his curiosity sharpened by his own first careful determination of molecular weights, or his first "Grignard" preparation.

Research work in the sciences is necessarily narrowing. If the undergraduate student is working in the laboratory with a graduate student or a professor it appears to us that the undergraduate student would be hindered by this limitation. Is it just to the undergraduate student to train him for months in a narrow, highly specialized field? The student should not be exploited in hack work.[3] Fairness to the undergraduate student seems to point to more varied techniques and instrumentations that should be found in his course work.

Another factor that should be considered in the crowding of the undergraduate students with so-called research courses in chemistry is the fact that the chemist is a member of society and should be trained to live as a member of that society and get along with his fellow human beings. Perhaps some of his few elective subjects should be along nontechnical lines in the humanities. The American Chemical Society has encouraged members to participate more actively in national and local politics and community endeavors. Chemists trained as critical unbiased observers can do much to safeguard our democratic principles for posterity. Perhaps a course or two in the social sciences for the undergraduate student will help him to be a better member of the society that nurtured him, to which he belongs, and to which he owes an obligation.

[1] Present address: Arizona State College.

[2] KIRK, R. E., "Research for the undergraduate," J. CHEM. EDUC., 9, 280 (1932).

[3] HURD, C. B., "Some observations on undergraduate research in chemistry," J. CHEM. EDUC., 21, 81 (1944).

MEASUREMENT OF THE HALF-LIFE OF UX_2[1]

A Lecture Demonstration

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FUNDAMENTAL techniques of radiochemistry can be demonstrated using only the naturally occurring radioisotopes normally available in uranyl salts and a simple Geiger counter. In the purification of uranyl salt reagents the decay products of uranium are removed, but the first two products of the decay, UX_1 (thorium), and UX_2 (protactinium), quickly grow in again. They reach equilibrium in a few months. The next member of the series, U_{II}, is very long lived, so that the series is, in effect, arrested at this point. The uranyl salt obtained from a chemical supplier will contain the equilibrium amount of UX_1 and of UX_2 (and its isomer UZ), but no appreciable amounts of the other members of the decay series.

It is quite easy to separate the UX_1 from the uranium by means of a carrier precipitate. But to perform the experiment described later the UX_1 must first be isolated in a weightless (carrier-free) form. The following procedure has been found to give a cleaner separation than other proposed methods, and to be more conveniently carried out. A quantitative yield is not obtained but this is not required. The new point in the method is that use is made of the fact that UX_1 forms a so-called "radiocolloid" in nearly neutral solutions that do not contain too high a concentration of electrolytes. This radiocolloid, which is probably thorium hydroxide formed by hydrolysis, adsorbs strongly on glassware, especially when the solution is heated.

Preparation of a Carrier-free UX_1 Source. About 10 to 50 g. of uranyl nitrate hexahydrate crystals are dissolved in ether. A stoppered flask is used since there is danger of separation of an insoluble solvated salt by evaporation of the etherial solution. The small aqueous layer formed from the hydrate water contains almost all of the UX_1 and can be separated by centrifuging the solution and decanting the ether. The aqueous residue is evaporated with a few drops of concentrated nitric acid until crystals start to separate, and then the ether extraction is repeated on a smaller scale. To separate the UX_1 from the small amount of uranium remaining, the residue is diluted with 2 per cent nitric acid, 5 mg. of lanthanum nitrate carrier added, and the lanthanum and UX_1 precipitated with oxalic acid. The lanthanum precipitate is separated and ignited to oxide, then dissolved in nitric acid and evaporated to dryness on the steam bath.

The residue is dissolved in 75 ml. of water in a 100-ml. beaker. The solution, which should have a pH of not less than 5.0, is heated nearly to boiling for one hour. A sizable fraction (40–60 per cent) of the UX_1 adsorbs on the wall of the beaker, leaving the lanthanum in solution. The beaker can be washed with water without any loss of the UX_1, which is then stripped from the walls with nitric acid. A portion of the nitric acid solution is evaporated on a platinum tray (crucible lid) and used as the source in the following experiment. The residue on the tray should be invisible, or nearly so.

Determination of the Half-life of UX_2. The UX_2, which has a half-life of 1.14 minute, is in equilibrium with the parent UX_1, whose half-life is 24.5 days. The observed activity of the equilibrium mixture decays with the half-life of the parent. But half of the beta particles really come from the UX_2, as can be shown by placing a series of aluminum absorber strips between the source and the Geiger tube. There is seen to be a soft component and a hard component, for the UX_2 beta rays are more penetrating than those of UX_1. To demonstrate the decay of UX_2 it must be separated from the UX_1, collected in a suitable form, and inserted in the counter in a very short time. No very rapid method for doing this has been reported and it has hitherto been necessary to start with a rather large quantity of material in order to have enough activity remaining at the start of counting.

The method now described is so simple to carry out that it can be done very readily in a classroom. The separation is clean and rapid and made directly on the counting planchet. It does not give a completely quantitative recovery but this is not necessary. It gives a 50 per cent recovery of the UX_2 originally present and takes less than one minute to carry out, so that one need start with only four times as much UX_2 as one wishes to have remaining at the start of counting. The method was suggested by an observation of J. W. Knowles[2] who found that Pa^{233} is difficult to remove from platinum ware after an acid solution has been evaporated. Thorium so treated is readily soluble. Protactinium oxide is known to be but slightly soluble in acid, but it is perhaps rather surprising that this fact can be exploited when dealing with amounts of the order of 10^{-10} g.

The UX_1/UX_2 source, on a platinum tray, is covered with one or two drops of concentrated nitric acid and warmed gently to dissolve the UX_1. The UX_2 also

[1] Presented at the Annual Meeting of the Chemical Institute of Canada (Chemical Education Subject Division) June 21, 1950.

[2] KNOWLES, J. W., Ph.D. thesis (McGill) 1948.

dissolves, for that which is present was formed from UX_1 in the dry residue and so had no chance to adhere to the platinum. The following operations should now be carried out in the minimum possible time. The nitric acid is evaporated to dryness on a hot plate (about 30 seconds). The tray is immediately dipped into concentrated nitric acid for one second, then plunged into a beaker of distilled water to rinse off the acid. It is quickly withdrawn, blotted with filter paper, and placed in the counter. Almost all the UX_1 is dissolved off by the acid but about 50 to 70 per cent of the UX_2 remains on the tray.

The decay of this UX_2 is readily apparent and can be plotted by students if the scaling circuit used gives audible clicks or has indicator lights. The curve should be plotted on semilog paper. After complete decay of the UX_2 a relatively small activity due to undissolved UX_1 will remain. This amounts to less than 2 per cent of the original. When it is subtracted from the other activity readings, the resultant curve is a straight line from which the 1.14-minute half-life of UX_2 can be read off. The separation yield can be determined by projecting the decay curve back to zero time; *i. e.*, the time at which the separation was effected. This can be taken as the time at which the solution was just completely evaporated. This extrapolated value is then compared with the original UX_2 activity, which is the value given by the UX_1/UX_2 source when the soft beta rays of UX_1 are screened out with aluminum foil.

The experiment can be used to illustrate some fundamental points of radiochemical technique, including the analysis of mixtures of activities by the resolution of absorber curves and decay curves, and it serves as a good introduction to a discussion of decay rates and secular equilibrium in radioactive chains of decay.

A VERSATILE GAS-ABSORPTION APPARATUS

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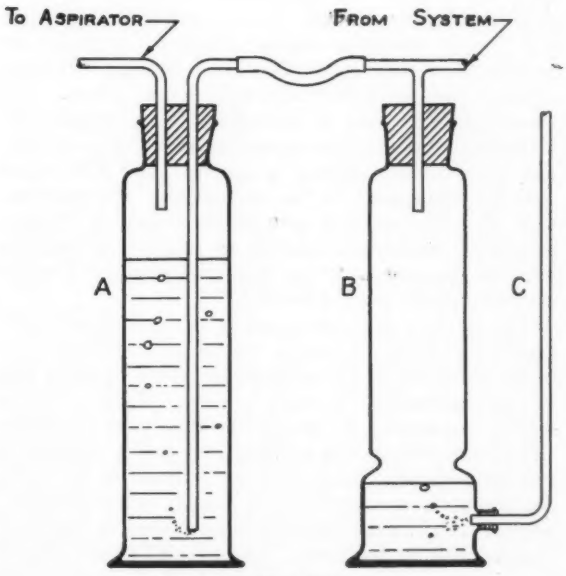
THE author wishes to call attention to the not necessarily original apparatus illustrated in the accompanying diagram. The arrangement has been successfully employed in our laboratory for the adsorption of noxious gases which can be readily absorbed in liquid media. Its utility is apparent from its simple construction from materials usually available in all laboratories.

As diagramed, the container *A* is an absorption tower and *B* a conventional drying tower. The absorbing liquid is filled to some convenient height in *A* while the liquid level in *B* is only high enough to cover the lower end of the tube *C* by a cm. or so as usually operated.

The function of the liquid in *B* is to act as a seal. When the aspirator is pulling a vacuum so that bubbles rise in *A*, the pressure will be reduced in *B*. When the pressure is reduced below atmospheric pressure by an amount equal to the liquid head in *B* air will enter through the tube *C*. If the evolution of gas in the system is more rapid than its evacuation through *A*, the liquid in *B* will rise in the tube *C*, thus increasing the pressure. The pressure increase will cause more rapid gas passage through *A* until equilibrium can be established. If the pressure builds up too rapidly, *C* acts as a safety valve by allowing the liquid in *B* to be expelled through it until the difficulty can be remedied.

This apparatus has been found very satisfactory in our laboratory for the absorption of hydrogen chloride generated in Friedel-Crafts reaction, sulfur dioxide-hydrogen chloride mixtures from reactions with thionyl chloride, and excess ketene from acetylations using the gaseous reagent. In all of these applications aqueous caustic solution worked very nicely as the absorbing liquid. If very large quantities of gas were absorbed so that heat became an item, the tower *A* was replaced by a larger container, placed in a trough, and water run over the exterior at a rate sufficient to remove the excess heat.



Gas-Absorption Apparatus

AN HISTORICAL APPROACH TO THE TEACHING OF SCIENCE[1]

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THAT there is ample room for improvement in public understanding of science is only too painfully apparent in a country that spends more on its astrology than on its astronomy. Many of us have been pleased to see science finally beginning to play some role in the shaping of national policy, but there has been a tendency to overlook the almost inevitable corollary—that national policy will play an increasingly important role in shaping the course of science. We have been distressed to see, in other countries, the tragic results of a public policy that is hostile to, or at least suspicious of, science and the scientist. We cannot blithely assume that science in this country is entirely free from such dangers, some manifestations of which can, indeed, be seen already. The biological sciences have already felt the sting of public disapprobation: the teaching of evolution has been hampered by legal restrictions in some states and, owing to the machinations of antivivisectionists, many medical research institutions lack an adequate supply of experimental animals. Presumably social pressures have become active in the biological domain somewhat earlier because the issues involved seem more immediate and because everyone assumes that he knows something about them. But there is now no lack of immediacy in the issues associated with modern physics and chemistry in particular, and with modern science in general; and with every man his own expert on "atomic energy," the intervention of public opinion and public policy in "purely scientific" matters is already a reality. Something must be done to ensure that this intervention will not be a destructively painful reality.

The public's misunderstanding of, and occasionally positive antipathy towards, science and scientists is a bitter reflection on the methods by which we have previously attempted to teach science. There is every reason to consider the possibility that some nontraditional approach to this problem may prove superior to the methods that have been our reliance in the past. Some aspects of a particular approach to this problem, worked out in a general education course at Harvard College, will be discussed. The over-all philosophy of this course is that expressed in J. B. Conant's "On Understanding Science," and involves the presentation of science as an intensely human endeavor that displays fairly clearly distinguishable and strongly persistent elements—elements dealing with the dynamics of its progress, its various bases, its capabilities and limitations, its relations with technology, economics and philosophy, and its influence on, and the way it is influenced by, social movements and pressures. Our pedagogic strategy is based on an attempt to steer a middle course between two extremes that seem almost equally unsatisfactory:

(1) We do not teach purely for their own sake the facts and theories of science, which are all too soon forgotten by the nonconcentrating student. We try not to confuse the character of science with the content of science. We do not suppose that the teaching of, say, the various oxyacids of sulfur presents any royal road towards the understanding of science. We teach the facts and theories only in so far as they are helpful in reaching some understanding of science.

(2) We do not attempt to abstract those persistent aspects that we believe we see in the scientific enterprise from the attendant raw material of fact and fancy that forms their context. We do not see great value in the fabrication (in a double sense) and inculcation of a relatively abstract "scientific method" in a pat, capsulated form. Indeed, such a presentation tends to encourage rather than discourage confusion of genuine scientific investigation with work like Velekovsky's, which has all the outward trappings but little of the content of a serious scientific inquiry.

Perhaps we can liken the condition of a well-developed contemporary science, like chemistry, to one of those ingenious puzzles by which we are challenged to dismantle a body that is presented in the form of a smooth and apparently unbroken sphere. Courses primarily intended for concentrators are quite often confined to a minute description and analysis of the various surface features of the contemporary science, and the interrelationships of these surficial (contemporary) facts and theories. This knowledge is unquestionably valuable to the concentrating student, but it is much to be doubted that the nonconcentrator can secure from such considerations any real understanding of the internal structure and almost organic development of science. A second alternative which, for lack of a better term, we may designate as the philosophical approach devotes little attention to the surface features of the contemporary science and directs its main effort to the description of a quite *ad hoc* idea of what the internal structure of the body of science *ought* to be like. A third possibility, which seems to correspond to our own approach to the puzzle, is perhaps best described as a partial dismantling of the whole structure, followed by a most painstaking examination of the surface and solid formation of a number of representative pieces and a consideration of the relations that exist among these frag-

[1] Presented at the 118th meeting of the American Chemical Society, Chicago, Illinois, September, 1950.

ments. We make no attempt to "cover" the whole field of the science (the very word "cover" suggests the superficial treatment which is all too often involved in "survey" courses), but we do try to present an intensive treatment of a number of illustrative instances that seem to us to be significant and helpful in gaining some appreciation of the character of the scientific enterprise. We examine the content of science in part, with the goal of revealing something about the methods of science; we study the methods of science, but only in the context of the facts and theories with which they are associated.

In selecting the fragments or "blocks" to be studied we do not seek out those that form a maximum part of the outer surface of our sphere of science. They would involve a time-consuming presentation of a great deal of factual and theoretical material that is not of value in and of itself. Instead, we focus attention on fragments that—though they may not contribute much directly to the total surface of the contemporary science—form particularly important and readily understandable parts of the underlying organic structure of that science. In short, we stress a number of key historical episodes in science, developments that can be understood fairly easily without a great background of fact and theory, yet developments that have played a most important part in the elaboration of the total structure of science. If, as it seems to us, it is justifiable to suppose that there are a number of features that have characterized "modern" scientific endeavor, from the time of Galileo to the present, then there should be no objection to the consideration of these continuing elements in the simpler, if more remote chronologically, contexts.

To date science teaching has largely neglected, oversimplified, or willfully misread the rich legacy of scientific history. The "scientific method" that is so frequently mentioned appears, for the most part, to be a free intellectual creation that bears but a shadowy relation to the ways in which science has actually progressed. As usually considered, the "scientific method" is compounded primarily of a number of almost purely deductive and technique operations of science. Important as these are, they cannot be left to stand by themselves. Surely some attempts must be made to point out the enormous, essentially intuitive, inductive leaps that have marked scientific progress. Surely some attention must be devoted to the usually neglected but really challenging aspects of scientific endeavor—such as the "simple" matters of recognizing what the problem is; of learning what to measure, how to measure it, and what to do with the measurements; of devising reasonable hypotheses and convincing experimental tests for their examination, etc., etc. To the extent that these matters are slighted a distorted picture of science will inevitably be produced, for these matters are the very bone and body of science. The neglect of scientific history has resulted in the neglect of material that is genuinely important to an understanding of science, and the misreading of history has fostered ideas about science that are sterile at best and misleading at worst. A few examples will illustrate this point.

Everyone is familiar with the account of Galileo's public demonstration, at the tower of Pisa, that objects of different weight fall at the same speed. Pedagogically this story appears to be of great value: it suggests the so-called "crucial" experiment. It suggests that Aristotelian physics was overthrown and modern physics created, in one fell swoop, as soon as man turned from "idle speculation" to "dispassionate observation." It is, indeed, a charming story and contains but one slight flaw: as far as we can discover, it never happened. Even more embarrasing, it appears that Galileo in no sense disparaged the power of speculative reasoning and that he had rather less regard for experimental demonstration than had some of his foremost Aristotelian opponents. The whole story, and the conclusions that have been drawn from it, are entirely illusory.

DEVELOPMENT OF OXYGEN THEORY

Fables of this sort are no less prevalent in chemistry than in physics. For example, consider the explanations usually given of Lavoisier's success in developing an oxygen theory of combustion. Some say that Lavoisier was simply the first to hit upon the idea that the atmosphere was involved in combustion. More popular is the notion that Lavoisier was the first to use the analytical balance, and hence the first to secure the accurate weight data (specifically, the weight gain on calcination of metals) required for the foundation of an oxygen theory. Thus it is suggested that Lavoisier hit upon the right idea and/or that he secured some vital, brand new data and then—all at once—the oxygen theory sprang full-grown as Athena from the head of Zeus. There is, indeed, some relation in the fabulous character of both of these stories. Misapprehension of the real history of the oxygen theory is so widespread, and the real history is so exciting and revealing, that it may be worth while to examine the situation a bit more carefully.

Lavoisier was by no means the first to apply the analytical balance to chemical studies. Accurate weight data on chemical reactions had been obtained more than a century before his time. Furthermore, Lavoisier was not the first to note the weight gain accompanying the calcination of metals. A qualitative appreciation of this effect had, indeed, been secured about a millennium earlier; and some quantitative measurements of the increases in weight characterizing the calcination of a variety of metals had been available for more than a century before Lavoisier's work. These data were known to Lavoisier's predecessors and contemporaries; and though Lavoisier extended them, they were in no sense his discovery.

The weight changes occurring during calcination were not the only data with an important bearing on the oxygen theory that were available before the last quarter of the eighteenth century. For example, it had been known for some one and one-half millenniums

that combustion in a water-sealed container was followed by a rise of the sealing fluid—indicating, perhaps, a "consumption" of part of the air originally present. The very large volume contraction following the combustion of phosphorus under these circumstances had been remarked by Stephen Hales, early in the eighteenth century. It was further recognized that, with the exception of a few special instances, combustion and calcination could not long proceed in a limited volume of air, and that they were completely arrested when the materials involved were placed in vacuum. The exceptions were materials (like gunpowder) that were composed of a combustible and niter. In the latter part of the seventeenth century the study of these special cases led Boyle, Hooke, and Mayow to the further observation that substances burned brilliantly on fused niter, even in the absence of air; and, in several instances, the products of combustion on niter were shown to be essentially identical with those produced by the analogous combustion in air. It was also known that on heating niter a volatile spirit was formed; but, although oxygen was prepared by this method almost a century before the time of Lavoisier's work, nobody thought to examine this volatile spirit's particular properties. There were many other suggestive observations with great relevance to the oxygen theory, yet somehow all these "facts" counted for nought for more than a century. This situation is markedly different from the idea of science that is all too often encouraged by conventional science teaching: an idea of "collect the facts and let the theories take care of themselves." The facts, a great many of them, had been collected years before Lavoisier's time, yet there was no orthogenetic production of an oxygen theory.

Can this failure be explained by saying that it was simply a case in which nobody before Lavoisier came anywhere near the right track? Might it not be, for example, a case in which it had not even been recognized that a problem existed? Neither of these suggestions can be defended. The phenomena noted above excited very widespread attention, and almost a century before Lavoisier's work his countryman Lemery particularly remarked on the singularity of the weight gain accompanying calcination, saying:

> In the calcination of lead and of several other substances there occurs an effect which well deserves that some attention should be payed to it. Although by the action of the fire the sulphureous or volatile parts of the lead are dissipated, which should make it decrease in weight, nevertheless after long calcination it is found that instead of weighing less than it did, it weighs more.

Not only was there interest in the effect, there were some reasonably close approaches to what is now considered to be the right explanation. Thus, almost a century and a half before Lavoisier's work, Jean Rey suggested that the gain of weight in calcination was due to a fixation in the calxes of some part of the air. The intervention of the air in combustion and calcination was also suggested by the observation of the apparent contraction of volume that accompanied these processes when they were conducted in water-sealed vessels. Commenting on this point, Fludd, one of the later English *alchemists*, remarked:

> Water enters the flask in proportion as the air is consumed ... for air nourishes fire, and in nourishing is consumed.

This statement dates from the early seventeenth century. But even in the fifteenth century it had been remarked by Leonardo that:

> The element fire consumes continuously the air as concerns that portion which nourishes it, and there would be formed a vacuum if other air did not come to supply its place.

Late in the seventeenth century this conclusion was further strengthened by Hooke's studies of the structure of the candle flame. At the same time, having observed that the glow of phosphorus was soon extinguished in a sealed tube, Boyle said:

> The air has some vital substance preyed upon or else tamed by the fumes of phosphorus.

Certainly it cannot be denied that the intervention of the atmosphere in combustion and calcination had been fairly clearly suggested. Through the labor of John Mayow, who died about a century before Lavoisier's work, this general perception was made the basis of an elaborate theory. Mayow supposed that the atmosphere contained certain nitro-aerial particles (so named because the same particles were supposed to be present in niter) which were involved in combustion, calcination, respiration, and so forth. Since the same particles were supposed to be present both in air and in niter, the identity of the products of combustion in air and on niter was easily understood. It also became possible to explain why niter was able to replace air as a supporting agent in combustion and calcination. Furthermore, since the nitro-aerial particles were supposed to be consumed in these reactions, it was plain why they would proceed only to a limited extent in a limited supply of air, and not at all in a vacuum. Yet again, since it was assumed that the nitro-aerial particles were associated with but one part of the air, there was no difficulty in comprehending why combustion was extinguished before all the available air was consumed. Finally, having devised a thoroughly elegant and original experiment by which the reaction of air with nitric oxide, in the presence of water, could be followed, Mayow showed that about one-quarter of the air was different from the other three-quarters. The proportion is the same as that first stated by Lavoisier.

Mayow was sometimes rather muddled in his thinking, and he certainly overextended his doctrine. Nevertheless, it would be a flat contradiction of history to say either that Lavoisier was the first to secure the experimental data necessary for the foundation of an oxygen theory, or that he was the first to conceive of the idea that forms the basis of that theory. However, if we disallow these overly facile explanations of why Lavoisier was so strikingly successful where his predecessors had failed to impress, we face the problem of providing a superior explanation. This explanation is

perforce somewhat more complicated, but it is an infinitely more exciting story than that usually given, and one which appears to have very great relevance to any study of the nature of the scientific enterprise.

What of the contention that the headlong progress of modern science is a very different thing from the faltering steps made by a primitive science, so that a study of the latter throws comparatively little light on the former? It seems that this point is, to say the least, arguable. We have observed that after many of the basic facts and ideas became available there was about a century's delay in the fruitful development of the oxygen theory. Is there any essential difference between this hundred-year delay and the approximately five-year delay in the recognition of nuclear fission as something distinct from the creation of transuranic elements? Considering the enormous weight of the experimental resources brought to the study of the neutron bombardment of uranium, the five-year confusion in the modern case seems, if anything, more striking than the century of confusion in the older case. Furthermore, a detailed study of the primary publications relating to both issues provides strong indications that the intrinsic character of the retarding influences in the two instances was not essentially dissimilar. Plainly an understanding of the factors that delay or promote the progress of science is of the utmost importance to today's intelligent citizen; and, conversely, the citizen's understanding is of great importance to the contemporary scientist. Consequently, it is our opinion that a detailed consideration of the dynamics of a typical, even if historically remote, scientific advance is very well worth the considerable expenditure of time that it entails.

REASONS FOR DELAY

The present discussion can provide no more than a brief sketch of some of the more important factors involved in the delay in the development of the oxygen theory. Certainly Lavoisier's work was accomplished in the light shed by Black's classic study of the composition of carbon dioxide, which did not appear until 1756. However, it appears that there were other more subtle yet more important reasons for the failure of all earlier attempts to create and popularize an oxygen theory of combustion and calcination.

First, it should be noted that to win acceptance a novel theory must be found generally consistent with the prevailing climate of opinion—particularly but not exclusively, scientific opinion. A century before Lavoisier's work an oxygen theory could not meet this test of consistency. For example, air was still regarded as element, so that the idea of a composite atmosphere was repugnant. But if air is to be regarded as a one-component system, then it becomes very difficult to explain, on an oxygen theory, why it is that a flame expires before all the air available to it is exhausted. The upholders of the phlogiston theory met no difficulty here—it was simply supposed that the flame expired when the air became saturated with the phlogistic exhalation(s) of the burning substance. And, indeed, the appearance of a rising candle flame, seeming to tug at the wick, strongly suggests that something is *leaving* the burning body. Then too, there was another prevailing belief that earths (*i. e.*, metallic oxides) must be elementary bodies. Surely the bodies that occur naturally have the best claim to consideration as elements. Furthermore, this idea was in accord with the observations of the working metallurgist that something (*e. g.*, charcoal) had to be *added* to the earths in order to convert them to metals. The rather striking similarities in the properties of the various metals were then easily understood since, on this basis, they would all contain a common substance (phlogiston) obtained from the charcoal. This scheme was also nicely consistent with the observation that when earths were heated with charcoal, practically all of the latter disappeared. Let us suppose that charcoal consists mainly of phlogiston which is combined with a little ashy material: then, as the phlogiston passes into the metallic calx, the latter is converted to the corresponding metal while the residue of the charcoal would only consist of a little ashy material. Here, then, the phlogiston theory appeared to be on very sound ground, while the fundamental idea of an oxygen theory—taking metals to be simple bodies and the corresponding earths as compound substances—was consistent neither with the obvious "facts" nor with prevailing scientific opinions.

To be sure, in its explanation of the weight gain accompanying the calcination of metals, the oxygen theory enjoyed a relatively advantageous position. However, the phlogiston theory was far from bankrupt in this particular respect. It was suggested that phlogiston might have a negative weight, so that as it was lost from a metal, in the conversion of the latter to a calx, there would be a net increase in the weight of the system. A related notion equated the loss of phlogiston (the metallizing principle) with the loss of a metal's "soul," leaving a metallic cadaver (a calx)—and it had long been supposed that a cadaver (a "dead weight") was heavier than the corresponding living body. Yet another possibility was that during the heating, by which the phlogiston was expelled from the metal, ponderable igneous particles from the fire lodged in the calx. It should be noted that this idea of a ponderable matter of heat was not unfavorably regarded by Lavoisier himself.

Although we say today that the observation of the weight gain in calcination was the "crucial experiment" by which the phlogiston theory was overthrown, history does not record that this experiment made any such dramatic impression on contemporary observers. The phlogiston theory remained much more generally attractive than the oxygen theory. It may be objected that in a more sophisticated scientific world these weight data *would* serve as a crucial indication. It may further be objected that the whole postulation of an almost purely spiritual subtle fluid, like phlogiston, is the mark of an outmoded style of scientific reasoning.

But is the era of the apparent triumph of a theory based on an all-pervasive luminiferous ether beyond the memory of living man? And was the theory dropped as soon as Michelson and Morley performed their "crucial experiment?" Apparently not. And was the neutrino anything more than a subtle matter postulated in a last-ditch attempt to save the mutual consistency of experimental data and the conservation laws to which we cling with such tenacity? If the neutrino should ultimately prove illusory, if the conservation laws should prove false, and if the teaching of science remains much as it is today—then no doubt our children's children will be taught that the discovery of continuous beta-ray spectra was, or would have been, a "crucial experiment," save for our stupidity in postulating anything as "obviously ridiculous" as a neutrino. Of course, we feel brilliant rather than stupid; but it seems evident that we are hardly in a position to deprecate the phlogiston theory as an "obviously ridiculous" notion, particularly while some of our most sophisticated physicists are seriously engaged in the discussion of the polarization of the vacuum—certainly a subtle fluid that must represent the ultimate in subtlety.

For the reasons noted above, and for many other reasons of equal cogency, the phlogiston theory was generally regarded as much more attractive than any oxygen-type theory. Some more or less direct experimental evidence pointing in the same direction was also available. Having observed the similarity between combustion in air and on fused niter, Boyle thought to recrystallize niter *in vacuo*, to remove its supposed aerial component. In this endeavor he was, naturally enough, unsuccessful, and the supposition that a common material occurred in both air and niter was made somewhat less obviously attractive. Of course, the experiment rested on a confusion of a physical mixture with a chemical compound; but, qualitatively, the misconception involved is not far removed from that which lay at the root of the recently prevalent delusion that the alkaline earth element formed by bombarding uranium with neutrons was radium rather than barium.

Another of Boyle's experiments had an even more unfortunate outcome. Having noted the increase in the weight of tin calcined in the open air, he attempted to conduct this calcination in a sealed vessel containing a limited amount of air. The retort burst forthwith, and Boyle perceived that a somewhat more elaborate procedure would be required. He then weighed the tin and retort together, before sealing the latter. Following this, the retort was heated to the maximum working temperature, and only then was it sealed. The heating was continued for some time, during which a small amount of tin calx was formed, after which the retort was cooled, opened and reweighed. A slight gain in weight was then noted. To be sure, on opening the vessel Boyle noted an inrush of air, but this he regarded as due simply to the replacement of the air that had been expelled during the preliminary heating, before the retort was sealed. Thus he was led to the conclusion that when tin was calcined in a sealed vessel, with a limited supply of air, the entire system experienced a gain of weight. He understood this as being due to the passage of ponderable igneous particles, from the fire, through the glass, and into the calx. This notion was nicely consistent with one of the explanations subsequently given by the phlogiston theory for the weight gain on calcination, and Boyle's work produced a corresponding weakening of one of the strongest arguments for an oxygen theory. A few years after the publication of this investigation, Cherubin d'Orleans pointed out the inadequacy of Boyle's demonstration, but most of the workers of the period accepted this demonstration at its face value.

A MODERN CONFUSION

Can one maintain that such an obvious misconstrual of experimental data could hardly occur except during the most primitive stages in the development of a science? A rather analogous instance occurred but a few years ago in the sophisticated field of astrophysics. The age of weakly radioactive materials can be determined from a measurement of the ratio between their helium content and their content of radioactive materials. On applying this method to a great many meteoritic and terrestrial specimens it was found that the maximum indicated age of meteoritic siderites was more than twice as great as that of any terrestrial material that had been examined. It appeared that the nature of the method was such that its most serious vulnerability to error (arising from a partial leakage and loss of accumulated helium from the specimen) would tend to produce too low apparent ages. Consequently, it seemed that if meteorites and the earth had been formed at the same time, the meteoritic age must be regarded as the more reliable estimate of the intervening period. There was a good deal of futile theoretical and experimental work on the discrepancy in ages, and some rather far-fetched possibilities were very seriously entertained. Yet, consider that during most of the period of confusion it was known that: (*a*) the meteorites would have had a particularly intense exposure to cosmic rays—an exposure much more extensive than that suffered in an equivalent period by terrestrial minerals, and (*b*) cosmic ray bombardment produces nuclear transformations in which alpha particles may be among the products formed. It is only about five years since these strong indications of a fruitful rationalization of the experimental data were recognized as tending toward the conclusion that there was *no* real inconsistency between the apparent ages of meteoritic and terrestrial materials. Instead of focussing attention on the possibility that the apparent ages of terrestrial minerals were too low, some consideration was finally given to the possibility that the apparent ages of meteorites might be too high. And, indeed, on determining the helium content at various depths in a large siderite, it was found that the helium concentration gradient was in a negative sense when proceeding from the exterior to the interior of the specimen. This

gradient is the reverse of that which would be expected to result from a leakage of helium from the specimen, but it is precisely that to be anticipated if a substantial proportion of the helium was produced by cosmic ray irradiation, rather than by radioactive decay processes. Thus a long-standing inconsistency was finally resolved, but only after very strong indications of the successful rationalization had lain fallow for a number of years. Are we in any position to say that Boyle and his contemporaries were stupid in missing an "obvious" explanation of one of the anomalies that confronted them?

FACTS VERSUS BELIEF

A very few of the points in our analysis of the long delay in the creation of the oxygen theory of combustion are suggested above. It seems plain to us that some of the factors which promoted that delay are closely related to influences that are still active in retarding modern scientific progress. Consequently, it appears that the consideration of some of these factors should be an integral part of any course in science for the nonscientist. We feel that it should be emphasized that the mere accumulation of facts does not constitute or guarantee a scientific advance. A given array of data can be variously weighted, evaluated, and interpreted, according to various theories. The ultimate choice of the most acceptable theory is an essentially aesthetic one that inevitably rests on a number of subjective judgments peculiar to the given scientific and social milieu. Experiments seldom, if ever, supply unequivocal answers, and the appraisal of the actual answers is subject to all the uncertainties and hazards of any purely human undertaking. Little attention was paid to Mayow's early and imperfect anticipation of Lavoisier's theory: an oxygen theory was then, by its very nature, conceptually unattractive—yet it might have been supported by fairly dramatic experiments. In our own day Ida Noddack's suggestion, in 1934, that the results secured by Fermi in the neutron bombardment of uranium could be explained as well (or better) on the hypothesis of nuclear fission as on the hypothesis of the formation of transuranic elements was almost completely ignored for five years. It then transpired that there were very simple and striking experiments by which fission could easily be demonstrated, but previously the whole notion had seemed so essentially implausible that it had not seemed worth while to make even a slight effort toward supporting it.

We believe that in calling attention to the difficulties, delays, and failures of science, as well as to its triumphs, we present a truer, better-rounded, and much more useful picture of it as it was in the beginning and as it is today. Most of this paper has dealt specifically with the growth of the oxygen theory of combustion, but many other cases present opportunities for similar analysis. One of the most notable examples of such a case is the development of the atomic theory. The account most frequently given suggests that Dalton arrived at his theory as a logical deduction from the existence of the laws of definite, multiple, and reciprocal proportions; that Avogadro used Gay-Lussac's "obviously important" combining volume data as the basis for an extension of the theory, and that from that time to this the theory has been triumphant. Yet the whole story, as so outlined, is little more than a tissue of fabrication.

Dalton's theory was formulated at a time when the law of definite proportions had come under serious fire, when the law of multiple proportions was as yet entirely undiscovered, and when the law of reciprocal proportions was almost certainly unknown to Dalton. Far from being a reasonable deduction from these laws, Dalton's theory was derived by a wild and almost completely erroneous line of argument based on physical phenomena that were largely irrelevant to a chemical atomic theory. Furthermore, Gay-Lussac's work was so little "obviously important" that Dalton flatly denied it. And Avogadro's Hypothesis was so little impressive that it was almost completely ignored for close to fifty years, until it was resurrected by Cannizzaro in 1858. Even more interesting, perhaps, is the fact that a little after 1840 there was a considerable body of scientific opinion to the effect that there were so many contradictions apparently involved in the atomic theory that it might be no more than a sterile speculation; and, among others, as acute a thinker as Ostwald maintained a skeptical attitude toward the atomic theory right down into the twentieth century.

Surely the reasons for these delays, for this skepticism, are an integral part of any satisfactory examination of the character and significance of the atomic theory. But the development of the atomic theory, as it is usually presented, is not an account of scientific progress as it was and is, but an indoctrination in the scientific prejudices and preconceptions of the instructor and the authors of the textbooks that he uses. No doubt such a presentation has very real values, but the course in which it is included should be called, honestly, "Natural Prejudices 1" rather than "Natural Sciences 1." To be sure, it is easier to construct an arbitrary imaginary account of a scientific development, that will well illustrate the points that are to be stressed, than it is to dig into the raw material of scientific history, to disentangle the shifting threads of fact and fancy that made that history what it was. However, a start in the historical research has already been made, for example in the new series of Harvard Case Histories in the Experimental Sciences which, if it is supported, will undoubtedly be extended to cover a larger part of our great heritage of scientific history. The history of chemistry, in particular, presents an almost inexhaustible series of profoundly interesting and important developments—developments associated with the names of Berzelius, Kekule, Mendeleev, and Arrhenius, to mention but a few. To date this historical material has been largely ignored, yet it appears that one of the brightest hopes for successful teaching of science in general (and chemistry in particular) to nonmajor students lies in at least a modest exploitation of this rich source of pedagogically valuable material.

PAPER CHROMATOGRAPHIC IDENTIFICATION OF THE AMINO ACIDS OF GLUTATHIONE

An Experiment in Biochemistry

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CHROMATOGRAPHIC procedures as research tools for the separation and identification of heretofore difficultly distinguishable substances have advanced considerably in recent years. The facility and simplicity of many of these suggested their incorporation into modern student teaching.[1] Whereas such student experiments involve identification of synthetic mixtures, it is possible at the expense of some additional effort to obtain more rewarding results with natural products and convey to the student simultaneously the satisfaction of duplicating in part the original researches. A technique involving the extraction of the tripeptide glutathione from baker's yeast and paper chromatographic identification of the constituent amino acids has been convincingly demonstrated in this laboratory by medical students working on a project basis for ten laboratory hours. The apparatus required is reasonably simple and the procedure straightforward.

Essentially, the procedure is conveniently divided into three parts: A concentrated extract of glutathione is made by precipitation of the copper salt[2] and decomposition with H_2S; the extract is hydrolyzed by heating with dilute HCl; and chromatographic identification of the three amino acids in the mixture is effected. Several simplifying modifications have been made in the original method for the isolation of glutathione with consistently reliable results.

EXPERIMENTAL PROCEDURE

Twenty ml. of concentrated H_2SO_4 is slowly added to 100 ml. of 95 per cent ethanol with cooling under running tap water. To the cooled solution 80 ml. of ethyl ether is added, and this solution is poured over 1000 gm. (or 2 lb.) of baker's yeast in a 2-liter beaker. After a minute of stirring the cold solution is filtered through coarse filter paper on a battery of three gravfilters arranged in the refrigerator with the funnel stems passing through the grated openings in the shelves into Erlenmeyer flasks sitting on the shelf below. (As alternatives, the solution may be centrifuged, or filtered with aspiration—the latter being the most tedious and difficult.) In this laboratory it was found convenient to allow the filtration to proceed overnight. The combined acid filtrates are adjusted to 0.5 *N* by first neutralizing to Congo red paper with 10 per cent NaOH and then adding the required amount of concentrated H_2SO_4. (This is accomplished by rough titration with the base, using a piece of Congo red paper swirled inside the flask to a color change from blue to red. The volume of concentrated H_2SO_4 required to make the solution 0.5 *N* is found from the formula: $x = 0.0135y$, where x is the amount of acid to be added; and y is the volume of the "near" neutral filtrate.) A suspension of freshly prepared cuprous oxide (about 1 gm./10 ml. of H_2O) is stirred carefully in 2-ml. portions into the extract at 50° on a water bath, care being taken to avoid an excess. The color of the oxide fades as it is added to the solution as in a volumetric titration; hence, an excess may be regarded as the first permanent pink coloration acquired by the solution. (Cuprous oxide is freshly prepared for this purpose by the slow addition of glucose to Fehling's solution followed with washing of the precipitate by decantation.) Gray cuprous glutathionate flocculates and settles out (15 to 20 minutes). A small excess of the oxide has not been found to cause re-solution of the precipitate and often stratifies above it. The amorphous precipitate is collected by centrifugation, washed with 0.5 *N* H_2SO_4 to remove excess Cu_2O, and washed with distilled water until free from sulfate (about five washings are necessary). The precipitate is suspended in five times its bulk of distilled water (about 5 ml.) and decomposed with H_2S which has been washed with a reversed wash bottle. The precipitated Cu_2S is removed by centrifugation or filtration, and the supernate containing the glutathione freed from residual H_2S by warming the tube, or better by the passage of a slow stream of nitrogen for about five minutes.

In order to identify the amino acids composing the tripeptide, the glutathione in solution is first hydrolyzed by refluxing gently for about an hour with 10 per cent HCl (1 ml. per 5 ml. filtrate). This is simply accomplished by using a test tube with a long piece (18 in.) of glass tubing as an air condenser (under the hood). Considerable HCl may be lost and the hydrolysis retarded if the solution is allowed to boil briskly. HCl has been found to be the most effective acid for hydrolysis because the resultant amino acids are best identified chromatographically as the hydrochlorides.

[1] GAGE, T. C., C. D. DOUGLAS, AND S. H. WENDER, J. CHEM. EDUC., **27**, 159 (1950).

[2] HAWK, P. B., B. L. OSER, AND W. H. SUMMERSON, "Practical Physiological Chemistry," 12th ed., The Blakiston Company, Philadelphia, **1948**, pp. 283–4.

Excellent details and clear drawings have been given for student participation in paper-partition chromatography of simple mixtures of amino acids by Gage, Douglas, and Wender.[3] The essence of their procedure is the same as that used in the present report, and either may be used. The method used is that of Consden, Gordon, and Martin,[4] as modified by Williams and Kirby.[5]

The correct dilution of the hydrolyzate for chromatography is found by placing on filter paper a few spots of different dilutions (1 : 10, 1 : 1000, and 1 : 1,000,000), quick drying in the oven at 80°, and spraying with ninhydrin, followed by another quick drying. From these, the spots of maximum intensity and hence the optimum dilution for the chromatograms is obtained. A penciled line is ruled on a sheet of filter paper (Whatman No. 1, $8^1/_2 \times 11$ in.) one inch from the edge and parallel to the long dimension. With fine-tipped medicine droppers solutions are spotted (about 1 cm. diameter) every 2 in. along the line. Solutions of 1 per cent glycine, glutamic acid, and an equi-volume mixture of the two are used as knowns (very few ml. of these are needed), as well as the solution of the glutathione hydrolyzate in the predetermined dilution. The paper is then rolled into a cylinder and stapled at each end. This is allowed to stand in a glass cylinder (9 in. tall and approximately 6 in. in diameter) the bottom of which contains about 50 ml. of phenol saturated with water. In order to insure saturation of the atmosphere, a small beaker containing water saturated with phenol is placed inside the paper cylinder. A large watch glass is used to cover the apparatus. The liquid front is allowed to climb for about 8 to 10 hours or sufficient time for a climb of approximately 8 in. The cylinder of paper is then opened and carefully dried at 80° for just sufficient time to effect drying without charring (about 10 minutes). Best results are achieved when this step is carefully supervised and the paper removed from the oven when it is just dry. A 0.1 per cent solution of ninhydrin (triketohydrindene sulfate in *n*-butyl alcohol) is then sprayed from an atomizer over the surface of the paper with care to saturate the paper evenly but to avoid excesses of liquid which could run on the surface and cause errors in interpreting the resultant spots. The moist paper is given a final drying at 80° for an additional 10 minutes or until the colored spots appear on the paper. The Rf values for the spots are found by measuring the distance from the starting line to the points of maximum density for the spots, and finding their fractions of the total distance which the solvent front has climbed, *i. e.*:

$$\text{Rf (rate of flow)} = \frac{\text{Cm. traveled by the amino acid}}{\text{Cm. traveled by the solvent}}$$

The use of two known solutions as standards, which are run simultaneously with the unknown solution, gives a convenient identification without taking the precautions usually necessary to duplicate the conditions required to get Rf values which agree with the literature. The spots derived from the unknown are compared with those of the known solutions (assuming that no other α-amino acids are present in the hydrolyzate). It has been possible to achieve consistently satisfactory results with values of the usual order and correct magnitude for the three amino acids.

Amino acid	*Rf (observed) 27 °C.*
Glycine	0.57
Glutamic acid	0.40
Cystine	0.14

The observed values are the average of three determinations which agreed within 4 per cent of one another, and for which a single student conducted the isolation and hydrolysis of the glutathione in two periods of four hours each. (A value for cystine instead of cysteine is presented on the assumption that the former is produced under the conditions of the experiment, as is frequently observed with hydrolyzates of peptides and polypeptides.)

Of eleven groups which have conducted the experiment, eight have been able to make positive identification of the amino acids present in the glutathione hydrolyzate; whereas one group obtained faint spots which did not correlate well with their standards and the remaining two groups were able to obtain chromatograms only with the synthetic mixture. Sources of trouble or failure were excessive drying of the chromatograms in the oven, the failure to use freshly prepared Cu_2O in the isolation step, and excessive handling of the chromatograms with resultant spots derived from impurities. However, it has been demonstrated that little beyond ordinary care will lead to fruitful and satisfying results.

[3] *Op cit.*

[4] Consden, R., A. H. Gordon, and A. J. P. Martin, *Biochem. J.*, **38**, 224–32 (1944).

[5] Williams, R. J., and H. Kirby, *Science*, **107**, 481 (1948).

WHY STUDY CHEMISTRY?

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THE students enrolled for a course in general chemistry can and do ask themselves the question: "What real use is this course to *me*?"

The answer for the student who intends to major in a science is obvious. His hope is to derive information to use as a tool in making a living. But there must be some justification for the presence in the course of the girls who major in home economics and the boys who major in english or commerce.

There *is* an answer, beyond the fact that the course may be required, or will provide the student with a certain number of hours toward graduation. It has been my custom to provide some part of this answer in a lecture sometime during the first semester. Experience has shown that the students are most interested in the discussion after about six or eight weeks of the course have passed.

The period of the teens and early twenties is a crucial one in life. It is during this time that the student is approaching intellectual and emotional maturity and is physically in his prime. It is also at this time that he is striving to discover what meaning and purpose there may be in life; he is about to come to the conclusion that other people actually exist as important entities rather than adjuncts to his personal welfare; and it is now that with freshly opening eyes he seeks to put the world into an understandable perspective. At no other time than this is the mind so supple and so capable of amendment.

If there is any real lack peculiar to the age it is the lack of all kinds of experience. Four years at college should provide the social and personal experiences to fill this gap. The first year at college may also be one of keen disillusionment, as the student discovers that the professors are not the gods who hold the final answers to all being and meaning in their august persons.

The particular and important service that the chemistry teacher can make is the contribution of facts to the building materials of personal philosophy. Certainly the best and finest house requires the most materials for its construction, and the most complete and satisfactory of personal philosophies must be housed in a framework of fact, however gaudily it may later be decorated with the furniture of speculation.

This we can do: provide the student with some ideas concerning the structure of matter, the finiteness of the kinds of matter, and the changes matter can undergo. To this, it is hoped, may be added an appreciation for the orderliness of nature and the simple laws which govern the infinite proliferation of her works. One should be careful to present known facts as the facts of experience and experiment, and stress the idea that the guiding light of theory must be confirmed by the bench of physical manipulation.

Certainly the student who has had one year of chemistry remembers virtually no detail after a few months. The things that stick will generally include some notion of atomic structure, and this information will be of some help to the individual in forming a rational philosophy and in understanding the very urgent implications of atomic power sources. But if this is to be all the student gains we may well be troubled. A good one-semester course in the nature of matter might be more to the point than a year of chemistry.

It is easy to attack any specific course as contributing little or nothing that the student will use after graduation. However, the huge reality and meaning of education lie precisely in that web of concepts and attitudes which have no meaning without factual detail. It is of no importance that the scaffolding of empirical fact disappear after the edifice we call education has been erected.

It is not wise to press the analogy too far. Beside this complex of attitudes and ideas we have called philosophy, or education, there is necessarily gained by the student a good deal of knowledge which might be characterized as off-hand knowledge in the sense that it is immediately present and usable whenever called up. Such is the knowledge the engineer uses in the practice of engineering and his information in the arts and literature which he may use in ordinary conversation. This immediately available information is tool and culture material and its acquisition is defensible.

But what of that information which has been lost, unused as other than scaffolding? The answer is that it has not been lost. Material which has sunk below the level of recall may be much more easily relearned and understood than brand-new material. The foundations have been built and lie in readiness for the possible time when a particular skill is to be developed. This non-recall information is thus not useless, even though a great part of it be unused. It might be pointed out that even tool information is largely sunk below the level of recall. Especially in chemistry, though a man have a million facts in his head, he still needs the reference literature.

The unique value of a course in chemistry is that it is the only course taken by the great majority of students in which they learn the structure and behavior of the matter of which their environment is composed. Such knowledge is of primary importance in the development of any realistic philosophy or the understanding of nature.

THE USE OF OBJECTIVE TESTS[1]

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THAT measurement is fundamental to science is a well-known fact. Measurement is also probably fundamental to sound educational development. This proposition is less widely accepted. Since the days of Sir Francis Galton psychologists have been attempting to measure and quantify differences among human beings, including differences in knowledge of many subjects. Objectivity is an essential characteristic of measurement whether the measurement be of electric current or understanding elementary chemistry. Thus we find objective tests have been developed in an attempt to make the measurement of individual differences more precise, more dependable, and more significant.

If we wish to measure the outcomes of education with as small an error as possible, and if we wish to place students at appropriate levels, we must use objective measures or make observations and check performance in some objective way. Certain rules of sampling and of measurement apply in educational measurement just as they do in the measurements common to chemistry. Unless reliable and valid measurements of educational attainment are obtained the results are useless as a basis for deriving an index of educational attainment.

Our interest here is in the educative process and if I understand my assignment correctly we are limited to paper and pencil tests. Performance tests—for example, analyzing a given unknown qualitatively and quantitatively, an objective exercise—are not to be discussed here, nor are tests of skill in the performance of other laboratory operations.

TYPES OF TESTS

Paper and pencil objective tests are used in the educational program in several capacities:

(1) For measuring aptitudes. The use of group objective tests for prognostic purposes was started in World War I, where the values of such measures were thoroughly established, and has grown extensively since then. At a low educational level we find today tests of "reading readiness," for example, to indicate when a child is best able to profit from the more formal work in learning to read. At the university levels we find widely used the scholastic aptitude test of the College Board and the psychological examination of the American Council, to mention but two of the most widely used aptitude tests from a very large field. The aptitude test attempts to indicate the intrinsic, constitutional, or dispositional fitness of the subject for undertaking successfully a specific kind of activity. In many cases ability to undertake a specific kind of activity depends upon previous knowledge and skills, and so aptitude and attainment are frequently both involved in predicting success at the higher levels. Skill and knowledge in mathematics, for example, are sometimes excellent predictors of success in advanced chemistry. The aptitude test result, if the test is a suitable one, is indicative of what a subject can do, with application, rather than what he will do under unspecified conditions.

Measures of aptitude are used today by industry in selecting personnel, by the civil service of both the federal government and the governments of many of the states in approving civil service ratings, and by institutions of higher learning. Rare indeed is the student who progresses to the college level who has not had his aptitude tested many times by objective tests.

Aptitudes are being subjected to analysis. Where a single intelligence test once served we now find several tests, perhaps of primary factors, substituted. In any case, the intelligence will be broken into a verbal factor and a quantitative one. As tests are broken into segments, each of which is refined and purified, greater skill and care are required for interpreting the results.

(2) In addition to measuring aptitudes, the measurement of attainment by objective tests is becoming more common almost daily. The measures of attainment too are becoming more complex as the goals of instruction are becoming more thoroughly analyzed. Where a single test might have served the purpose earlier now several tests are felt to be necessary. The course in elementary biology, for example, has many aims. Ability to handle the microscope is not necessarily related to ability to understand the biological principles involved in the course, or to memorize the many new names introduced. Thus we find several tests, each with its own score, are required. No doubt the same situation exists in chemistry. The ability to understand the theory of chemistry is probably quite different from the ability to weigh accurately or to do laboratory work skillfully.

Tests of attainment are used in the classroom at almost all levels. Standardized objective tests as they become available at the various levels are replacing less carefully constructed examinations.

Objective tests of attainment have found widespread

[1] Presented before the Division of Chemical Education at the 118th Meeting of the American Chemical Society, Chicago, Illinois, September, 1950.

use in the armed forces in determining qualifications for advancement in rating for the noncommissioned officer levels. They are used in civil service. Even tests for driver's licenses, I note, are making use of objective tests. The few tests of this type I have had an opportunity to examine personally were deplorably bad, primarily because they ask questions the answers to which do not signify ability to drive. Such achievement tests serve to force a review of rules but the significance of the rules for accident-free driving and the relationship between knowledge of rules and behavior is not always clear. A good objective test can be constructed for this purpose. Attainment tests of the objective type are also used in trade testing and in many other industrial uses. The educational uses of objective tests are manifold.

(3) A third type of objective test, in addition to the aptitude and attainment tests, is the objective test to determine the nature of one's interests. Success in any field is probably greatest where the individual has not only the aptitude and the necessary prerequisite attainments but also interests comparable to those who are successful in the field. Thus the measurement of interest has an important part to play. It is wasteful for an able and well-trained person, for example, to enter the long professional training to become a physician if he does not have a sufficiently strong interest in the profession to keep him at the task for the time required.

Unfortunately, at this time we have no dependable measures of interest which can be taken under pressure conditions where the candidate is attempting to convince an admission committee he is the right man. The very good objective tests of interest which we do have are suitable where the applicant is interested in cooperating with the examiner to discover where his interests lie.

(4) In addition to the objective tests of aptitude, attainment, and interests there are also objective tests of personality. The many inventory-type of tests which have been developed have found some use with the armed services and also with industry, as well as in the school situation, but these tests need further development before a more general use is indicated. They should be mentioned because they are treating an area that is of fundamental importance in the educational process as well as in almost all other aspects of life. That the adjustment factors play an important part, no one will deny.

There are many other types of objective tests, and the tests can be classified in different ways. Special objective trade tests, vocational tests, and the newer types of projective techniques are gradually being worked into objective group tests. Rather than using tests which measure only unimportant facts which are easily taught, quickly learned, and even more rapidly forgotten, objective tests are now being developed to measure the significant outcomes of instruction. The development of such tests and the analysis of the significant outcomes usually go hand in hand.

APPLICATION

How are these objective tests being used? Two broad categories of use are guidance and selection. Tests can be used to help in guiding individuals to their most suitable field or to select from a group the most promising for a specific job. If one has a job to do he attempts to select the individuals best fitted to help. The individual, however, tries to find the work for which he is best fitted and which to him is the most satisfying. Objective tests are used for both purposes. Selection is much easier than proper guidance. There are tests also used for a variety of steps in the educational process which do not seem to belong under the heading of either guidance or selection but rather education.

In the college situation, the selection techniques are used to determine who will be allowed to receive the training. The use differs in the selection process depending upon whether there are more seeking admission than can be trained, or whether there is need for more students than apply. In times of depression, now almost a forgotten era, selection is replaced by vigorous recruitment, and the stress is on measuring the ability to undertake the training at all. When the applicants exceed the places, the effort is to select the best qualified among the applicant group.

Objective tests are also used to select scholarship recipients from the large number of applicants. One outstanding program of selection based largely on objective tests is the Pepsi-Cola Scholarship Program in which 489 individuals were selected throughout the United States and their full tuition and fees, a travel allowance, and $25 monthly paid to them for their four years of college. A total of over $1,110,000 has already been paid to these winners selected finally on the basis of objective tests. Their records are more than justifying the method of selection.

Many other scholarship programs make use of objective tests. The Westinghouse Science Talent Search, while a much more modest program financially, has been in operation longer and is continuing. Here, too, objective tests play an important part in the final selection technique.

Many industrial plants have found that the routine use of objective tests with employees supplies evidence which can be used to bring about a more efficient utilization of personnel and to make a more satisfied group of workers.

In guidance work the tests are the basic tool of the profession. Without tests, guidance would be almost impossible. Guidance clinics and officers are growing. The use of these groups by the Veteran's Administration on a very large and broad scale has demonstrated their possibilities.

EXTENT OF USE

Thus far we have talked about the various types of objective tests and the ways in which they are used. Now let us consider how widely the tests are actually

used and just how they are used. Here we shall consider only the objective tests which are generally available; tests which are published and for sale either by the copy or as part of a testing service. The thousands upon thousands of tests of an objective nature made by the individual teacher for use with his own class are still another field.

If the number of objective tests of the type I have described which have been sold or used within the past years has been counted, I do not know the result. Unquestionably it runs into a great many millions. The Educational Testing Service, to take but one of several agencies, during the past school year scored and reported a total of 325,334 tests. In addition through their Cooperative Test Division they sold 1,243,823 copies of 353 different forms of tests which they offer. Civil service of the federal government is probably the largest user of objective tests during peacetime, and during wartime the armed services probably are the largest users. Their uses of objective tests will run into many millions.

Objective tests are available through commercial publishers. There are over twenty such publishers who issue catalogues devoted solely to objective test offerings. In addition there are at least seven universities, including state institutions, which publish test catalogues of their offerings of objective tests. The World Book Company, Houghton-Mifflin, Psychological Corporation, Science Research Associates, Bureau of Educational Research and Service of the State University of Iowa, Ohio State Department of Education, Public School Publishing Company, and Stanford University Press, all sell objective tests, to mention but a few of the many publishers of them.

In addition, a number of universities operate test construction divisions to prepare or help prepare tests especially designed for local uses. The University of Chicago, to take what is probably the outstanding case, has a Board of Examiners under the direction of Ralph Tyler. This group, working with the faculty, prepares the comprehensive tests used by the University. Max Englehart devotes himself to directing the preparation of objective tests and other tests for the Chicago Junior Colleges. While the budget for such groups is necessarily large, the results are believed to justify the costs.

Certain agencies also prepare tests for use with limited special groups. Such testing programs are usually given under supervised conditions, and the tests are not available for purchases or other use. The nonprofit Educational Testing Service, while a publisher of tests through its Cooperative Test Division, is also the largest group handling supervised tests in the country. The College Entrance Examination Board, whose tests are handled by E.T.S., has the largest nongovernmental supervised testing program as well as the oldest.

In addition to the programs of the C.E.E.B., the E.T.S. handles supervised programs for law students, the graduate record examination, national teacher examinations, West Point and Annapolis examinations, actuarial, national high-school program, pre-engineering inventory, Department of State Examination, Medical College Admission Test, National Research Council, and so on. In fact a total of 25 different supervised programs were handled this past year and administered at over 5000 different supervised centers.

The Educational Testing Service, while the largest, is not the only agency which handles testing programs. The Psychological Corporation has for many years developed and administered a program for selecting students to go into nurse training. The Science Service has developed and handled the test for the Science Talent Search. The American Dental Association handles its own testing program.

While these illustrations are not all inclusive, they are sufficient to show that objective tests are widely used.

COST

The cost of analyzing a course of study, of determining the aims, and devising the tests to measure the outcomes is substantial. Education—sound education—does not come cheap. George Bennett, president of the Psychological Corporation, has commented on the high cost of test construction and the relatively long time required for the validation of such tests. He says that the Psychological Corporation worked for two years in preparing its Differential Aptitude Tests designed for high-school guidance and spent approximately $75,000. He estimates another five years and an additional $100,000 will be needed to carry the program forward. Preparation of thorough significant tests is expensive.

The Educational Testing Service has a staff of 81 professional personnel, as well as the supporting clerical staff of 186, and the necessary equipment. Its gross expenditures for a year run into the millions. It has been developed to offer as expert services as are now possible at a rate as low as is consistent with the quality of the service. Its size, variety of services, and volume all contribute to efficient operation.

EVALUATION

Having by this time established that objective tests are indeed widely used by industry, the government, the schools, and various agencies; that programs of objective tests are also widely given under supervised conditions; and that test and examining groups of different types, sizes, and natures are in existence, a final word should be said about the tests and their significance.

As the use of objective tests has grown there has developed a cult of correlators who at their worst seem to believe that a simple correlation between a test score and some other measure on a small population can "prove" the value of the test. Certainly correlations should be made again and again. They should be made frequently enough so that one appreciates their fluctuations and recognizes that the size of a correlation is based on many factors, only one of which is the test.

A correlation is influenced by the nature of the population on which it is based quite as much as on the test. A test having thoroughly established intrinsic validity may yield scores with a given population of individuals which correlate to a slight extent only with some criterion. The test is still as good as ever.

Your interest and mine is in developing dependable, reliable tests of the more significant outcomes of instruction. This development of suitable, adequate tests is time-consuming and expensive in terms of brains and training which are demanded and of detailed item analysis of trial efforts. When the final test is ready and approved by both the teacher of chemistry and the test technician, who has applied his various techniques to show that the test is sound and dependable, then the test is of value for the purpose for which it was constructed. It has intrinsic validity. Use it and interpret the results wisely, for it will be a sound test.

AN AUTOMATIC RATING THESAURUS OF PHYSICAL CHEMISTRY QUESTIONS

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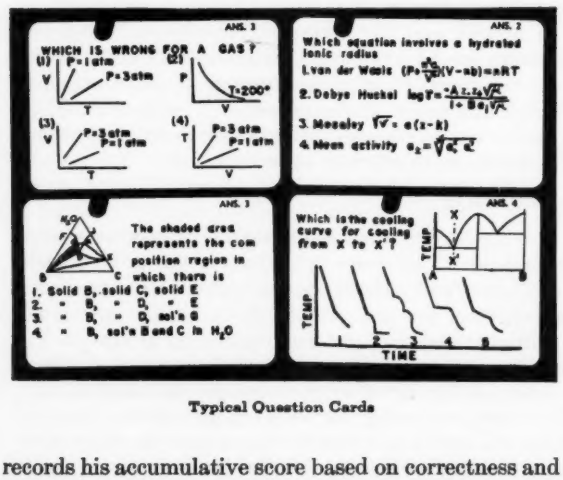
VARIOUS devices for supplying instructional material were investigated by the armed forces during World War II. One of the more interesting of these devices was developed by the Special Devices Center of the Office of Naval Research and is known as the 5-0 or 5-0-2 Automatic Rater.[1] This device presents multiple-choice question cards to the player and automatically



Typical Question Cards

records his accumulative score based on correctness and speed of selecting the answers. We became interested in whether the automatic rater would be helpful to students of physical chemistry and whether there would be any relation between the degree of interest in using the machine and the grade obtained by the student in the physical chemistry course. The scarcity of any information on the evaluation of these types of machines may be due either to the early state of development of the device or to a philosophy that such devices are undesirable in college-level teaching. The purpose of this article is to present briefly our preliminary evaluation of the rater made available to students on a non-compulsory basis.

The machine was loaded with cards containing physical chemistry questions and placed in the laboratory so that it might by played during any free time from the experimental work by sections totaling 150 students in physical chemistry. A lively interest was shown in the rater by the students. Each student played it at least once and filled out a questionnaire on the rater.

The automatic rater[2] displays multiple-choice question cards 2.5 × 3.7 in. and automatically records the player's accumulative score. The playing cycle is started by pressing a "start" button which causes a card to appear in the question window. Five seconds are allowed in which to read the question. There are seven answer buttons. The player presses the one corresponding in number to the answer considered correct. If the choice is made immediately and correctly a green light appears in the "correct" window and twenty points are scored. A penalty of one point per second results from delay in selecting the correct

[1] Due to the courtesy and interest of Commander R. W. Mayhew, U.S.N.R., Ninth Naval District Program Officer for the Research Reserve, a 5-0-2 rater was made available to us through the local Naval Research Reserve Unit.

[2] The raters have been manufactured by the Lewyt Corporation of Brooklyn, New York. When manufactured in lots of several hundred, the machines cost about $700 each.

answer. If the choice is not made immediately the twenty numbered lights go out successively, one every second. If the choice of answer is incorrect a red light appears in the "incorrect" window and no credit is given. After the answer button has been pressed the card moves to the answer window where the *question and correct answer* may be studied for any length of time. By again pressing the "start" button the cycle is repeated. The model 5-0-2 rater is loaded with about 500 cards at one time, and since the cards follow a continuous path through the machine the machine may be played as many times as desired without further attention except for maintenance. The model 5-0 rater is loaded with about 650 cards and each time the 650 cards have been played they must be manually reloaded into the machine.

Because of temporary mechanical failures and the work required to develop a suitable set of questions and have them transferred to the special cards used in the rater,[3] the machine was irregularly available to the students over a period of several months. No control was operated to test directly the value of the rater. An evaluation based on observation, personal discussions, and written answers to a questionnaire indicated that: (1) the students felt the rater was helpful in the course work, (2) there was no correlation between interest in the machine and grade received in the course, (3) the students desired to have the rater available during "library hours" in addition to laboratory periods.

Since the national standardized tests on physical chemistry utilize multiple answer questions (average reading and answering time is 92 seconds per question in the current edition), their use is apparently applicable in teaching physical chemistry. Automatic grading machines are becoming more common, although they generally will not correct the student's work as quickly[4] as the rater described. The use of the raters exploit an established American liking for machines[5] to encourage consideration of selected questions. The present rater does not have the optimum reading time but this could be changed and the reading time could be made variable for each question so that problems requiring calculations could be handled. The size of the question card might be increased.[6]

[3] The cards are made of a special paper composition to resist dimensional change under different humidities. The dimensions of the card are very important to the smooth operation of the machine and the cards have a tendency to jam the machine as they wear or are even slightly damaged. The recent adoption of plastic cards may solve this problem.

[4] DEMING, H. G., J. CHEM. EDUC., 26, 589 (1949).

[5] According to *Annals of the American Academy of Political and Social Science*, 269, 62 (1950), "the gross 'take' of all the slot machines in the country may be placed at one billion dollars per year."

[6] The Navy developed a 35-mm. film-projector rater which projects an $8\frac{1}{2} \times 11$-in. page question with multiple answers. This machine detects the correct answer by the use of photoelectric cells, whereas the 5-0-2 rater determines the correct answer by mechanical feelers which detect the position of a slot cut in the question card. For compactness it would appear that the film-type rater using perhaps 8-mm. film would have advantages over a card machine.

If it could be established that a machine is a sufficiently effective adjunct in learning, a rater could be designed as versatile as Vannevar Bush's memex[7] and might be built into the student's desk of the future.

Automatic Rater. Device 5-0-2

The rater may be used as one of the less arduous means of reviewing course material. It permits two or more students to compete with each other in a profitable way. Since all 500 cards in the rater may be different and can be changed as often as desired, the rater can encourage students to learn and apply principles rather than memorize details. The rater might be used as a minimal accomplishment device for a course, *i. e.*, each student might be required to make a certain score as a qualification to take examinations designed to give practice in analysis and self-expression with little emphasis on time. The rater may be used in any course in which multiple answer questions can be used effectively, and in the student questionnaire many courses such as freshman chemistry,[8] organic chemistry, geology, languages, etc., were suggested as ones in which the use of the rater could be helpful.

[7] BUSH, V., *Atlantic Monthly*, 176, 106 (1945); *Life*, 19, 112 (1945).

[8] BRUCE, P. L., J. CHEM. EDUC., 27, 277 (1950).

NOTES FROM THE LIFE OF A CHEMIST

PAUL WALDEN
Gammertinger-Tübingen, Germany
(Translated by Ralph E. Oesper[1])

THIS is a brief account of the rather unusual life story of a chemist-scientist, a self-made man, who from a penniless orphan gradually made himself into an internationally known scientist. This rise was due in great measure to my strong physical constitution combined with a life-long moderation in all pleasures (eating, alcoholic beverages, exercise, sports, etc.). On the other hand, during my younger period, I put no limitations on my scientific labors. Like Kekulé,[2] I can state that "for many years, four or even three hours of sleep were enough for me," and most of the night was devoted to experiments. Unconsciously, I also followed his other counsel: "I can only advise my young professional colleagues to be industrious in their youth." This self-discipline and voluntary restriction of pleasures and distractions paid me large dividends of time and energy, which I could expend on serious, diligent scientific work. Time for me was not money, but a divining rod, which led me to many hidden scientific treasures. My whole career has therefore been devoted to research, not with a concrete monetary return in mind, but because of the abstract pleasurable effects which are associated with the finding of a problem, the search for the means of solving it, and the thrill accompanying the successful solution itself. Every complex problem I have attacked has been like a drama; the last act always presenting a "happy ending."

BIRTH AND EARLY SCHOOLING

I was born on July 26, 1863, in the district Livonia, which then was part of the Russian Empire and is now included in Latvia. My father, Johann Walden, a farmer, and his wife Anna (neé Kreischmann) both died while I was quite young. After finishing the local German-speaking elementary schools, I, now an orphan, was sent to Riga, then the capital of Livonia. The Realschule (secondary school) there was quite new and modernly equipped. I made an excellent record, especially in science and foreign languages, and paid my own way by tutoring my fellow students. The technical university "Polytechnikum" in Riga where I matriculated in 1882 was famed throughout Russia because of its excellence in the sciences. The instruction was given in German. The young Wilhelm Ostwald (1853–1932) had recently been added to the faculty, and when I enrolled as a chemistry student I began an association with him that lasted until his death. He accepted a call to Leipzig in 1887, and I continued my studies in his world-famous laboratory during the summer semesters of 1890 and 1891. My doctorate was conferred *summa cum laude* at Leipzig in September, 1891.[3]

ASSOCIATION WITH OSTWALD

I was connected with the Riga Polytechnikum until 1919, as student, assistant, dozent, professor (1893/4), dean and, several times, rector. My assistantship began in 1885 in physics where I was lecture assistant. Along with these duties, I also started to experiment on my own and to look for problems. The latter were necessarily restricted to the chemicals that were available to me in the physics supply room: The lamps used to light the laboratory provided a supply of coal oil; the cleansing of the mercury for the physics experiments required concentrated nitric acid; several sets of Bunsen elements were available. Accordingly, in 1885/6, I was one of the first to attempt the nitration of petroleum hydrocarbons by means of concentrated nitric acid in bright sunlight. Occasionally, a white solid mass, with a lemon-like odor, separated; my intention to analyze it later was never fulfilled, unfortunately. The Bunsen batteries were to be used for the electrolysis of liquid hydrogen fluoride, but, while I was waiting for the delivery of the lead electrolysis apparatus I had ordered for this purpose, Henri Moissan isolated the free halogen.

Full of energy and ambition, I now applied to Ostwald for a problem, and he promptly suggested a study of the electrical conductivity of salts of polyacid bases. "Find out what there is in this," was the whole of the verbal instructions he gave me. I worked on this problem day and night (I lived in the physics building) from the fall of 1886 to the spring of 1887. I was perforce both apprentice and master, because in addition to the topic I had at my disposal nothing but my strong determination, my investigative enthusiasm, and my free day and evening hours—but no apparatus or materials. Though it was many years later that I became acquainted with Goethe's statement: "All living, all doing, all art must be preceded by work with the hands," I had unconsciously followed this maxim, in that I first of all took lessons in the mechanical arts: from the mechanic, the soldering of metals, lathe work,

[1] Professor Walden wrote these autobiographical notes at the express request of the translator.

[2] KEKULÉ, A., *Ber.*, **23**, 1308 (1890).

[3] Through the persuasion of J. H. van't Hoff, I became the first of Ostwald's students to write a biography of the revered master (1903/4). Later, acceding to the wishes of M. Bodenstein, I composed the necrology of the eminent teacher, scientist, and man, with whom I had been intimately associated for 50 years [*Ber.* **65A**, 101 (1932)].

etc.; from the glass worker, the cutting, blowing, melting, etc., of glass. In short, with my own hands I had to make the apparatus required for the Kohlrausch-Ostwald method of measuring conductivity: Wheatstone bridge, Arrhenius conductivity cell, measuring pipets, etc. Only the telephone, a resistance box, and the thermometer were borrowed from the physics supplies. These details are given here because of the great contrast to present-day conditions where research students are supplied with such items as ready-made precision instruments and automatic recording devices. Although times have changed, it is still well to emphasize the importance of being able to rely on one's own resources. Berzelius was able "to get along with surprisingly little" and if necessary "to bore with a saw." The great experimentalist Bunsen once told me that "lots of nice apparatus can be constructed even from empty cigar boxes."

In the spring of 1887 I submitted my completed manuscript to Ostwald with the timid request that he correct it. After several months he returned the manuscript, together with galley proofs from the *Zeitschrift für physikalische Chemie*, which is the periodical he had recently founded (1886/7). When I discovered that my paper did not contain a single correction by Ostwald, and when I soon held the printed copy of the journal in my hand—or rather, when I saw that my contribution accompanied those from the pens of the leaders in physical chemistry—I experienced for the first time the greatest and still undimmed thrill of the young scientist. In this way, the *Zeitschrift* [1, 529 (1887)] became the publication medium of my first investigation, which gave rise to the "Ostwald-Walden rule" for the determination of the valence of acids, n_S, or bases, n_B, from the increase in the molecular conductance μ between the dilutions $v = 32$ to $v = 1024$ liters, *i. e.*, $(\mu_{1024} - \mu_{32}) = 10 n_S \cdot n_B$. Thus, as early as 1887 my name was linked with that of Ostwald, both scientifically and through the *Zeitschrift*, in which in the succeeding decades (1887–1934) I published (Vols. 1–168) the results of my experimental studies of the ionizing power of nonaqueous solvents.

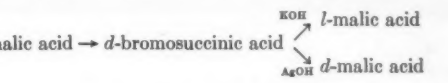
ORGANIC CHEMISTRY

Ostwald was succeeded by C. A. Bischoff, who came from Leipzig, and there was a decided shift in the chemical emphasis at Riga; physical chemistry gave way to a pronounced organic-stereochemical pre-eminence. I, too, changed my position, and (at a better salary) became assistant in organic chemistry. Although my days were largely occupied by my duties in the organic laboratory my nights were my own. I devoted my evening to physical chemistry, *i. e.*, I determined the affinity constants of numerous acids through conductance measurements. These experiments were carried out in my living quarters in the chemistry building.[4] At this period I was somewhat like the Roman god Janus, my daytime face was organic, my nighttime countenance physical chemical. I published organic papers jointly with Bischoff, and also my graduating study for the diploma of engineer-chemist (1889). My own publications in physical chemistry included the doctoral dissertation in Leipzig (1891) and the dissertation for the degree of Master in Chemistry at Odessa (1893).

I again took up organic chemistry in 1893 at Munich in Adolf von Baeyer's laboratory; I wished to gain a better insight into the mental and working atmosphere of organic and stereochemistry. Baeyer ruled like a high priest over his band of collaborator-students. At that time this distinguished company included: E. Besthorn, E. Bamberger, E. Buchner, A. Einhorn, W. Koenigs, H. von Pechmann, L. Vanino, R. Willstätter, and J. Thiele. The modern physical chemical methods did not enjoy much of a reputation among these great synthetists.[5]

"MAN GROWS WITH HIS OBJECTIVES"

To this may be added: "The goals and powers of a man increase with his successes." I became dozent in 1892 and professor of physical and analytical chemistry in 1893/4. Bischoff and I published a "Handbuch der Stereochemie" (1893). I then began an experimental investigation (1894 ff.) of the van't Hoff postulates of the asymmetric C atom and also an examination and refutation of the "asymmetry product" of optical rotation, set up (1890 ff.) by Ph. A. Guye. In the further course of these studies (1897–99) I discovered the well-known "Walden inversion."[6] For example:



I received the degree "Doctor of Chemistry" in 1899 from the University of St. Petersburg.

With the conclusion of these researches in the field of stereochemistry and optical activity,[7] I turned my attention to a new problem, namely nonaqueous solvents and ionizing media. I began (1899) with liquid sulfur dioxide and gradually extended my studies to all available inorganic and organic solvents, ending with hydrazine.[8] In a lecture delivered by invitation before the French Chemical Society (May 28, 1930) I gave a survey of the conductivity of typical binary salts in nonaqueous solutions.[9] My career as an

[4] In his memoirs, Ostwald refers to his own (1889) affinity determinations of hundreds of acids and points out that mine had surpassed his in numbers.

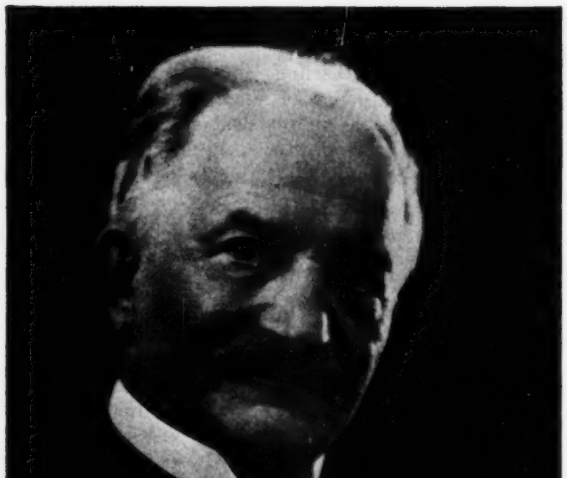
[5] For instance, when the great Emil Fischer once visited his revered teacher, Baeyer proudly displayed a "new, particularly fine piece of equipment," which, to Fischer's amazement, turned out to be nothing but a test glass held in a movable stand. On the other hand, it was reported of E. Fischer that he looked at every compound to see what its molecular weight was.

[6] In 1898 I underwent a "personal inversion," *i. e.*, I married Wanda von Lutzau. Our happy life together was terminated by her sudden death in July, 1950.

[7] WALDEN, P., *Ber.*, **38**, 345 (1905).

[8] WALDEN, P., *Z. physik. Chem.*, **165**, 241 (1933); **168**, 419 (1934).

[9] WALDEN, P., *Bull. soc. chim. France*, [4] **47**, 1–46 (1930).



Paul Walden

active laboratory worker closed when I reached the official retiring age in 1934.

When I began my measurements, around 1900, the molecular conductivities μ_v obtained with electrolytes chosen at random (*e. g.*, LiBr, $AgNO_3$, $SbCl_3$) were regarded as a measure of the ionizing power of the solvent. With the choice of a typical "strong" binary salt having pronounced solubility, and with the measurement of the molecular conductivity—down to the limiting value μ_∞ at infinite dilution V—at various temperatures t, in various solvents with different viscosities η_∞, I discovered the relation $\mu_\infty{}^0 \cdot \eta_\infty{}^0 = \mu_\infty{}^t \cdot \eta_0{}^t = [\mu^t \cdot \eta_\infty{}^t + v^t \cdot \eta_\infty{}^t]$, in which v and u denote the migration velocities of the free anion and cation, respectively. This is the Walden viscosity rule (1906).[10] As slightly solvated "normal salts," we have for example $N(C_2H_5)_4I$, $N(C_5H_{11})_4I$, or $N(C_5H_{11})_4$ picrate. Furthermore, it was found that, in general, there are no "indifferent" solvents.[11] Molecular weight determinations of $N(C_5H_{11})_4I$ in benzene indicated a high degree of polymerization.[12]

ACTIVITIES AND TRAVELS

Since 1900 my personal experiences have been set in a turbulent atmosphere; I, the quiet optimist, was plunged into the turmoil of the Russo-Japanese War (1904–1905), World War I, the Russian Revolution, and then World War II. As rector of the Riga Hochschule, I went through the Russian student uprisings, and these required conferences with the Minister of Education in St. Petersburg. To be sure, this office changed hands so often that the old *portier* at the ministerial palace resignedly remarked: "The ministers formerly moved in with furniture vans; now they come with no more than a handbag." D. I. Mendeleev, N. Menschuktin, and I collaborated in laying plans for the organization of a technical academy and university.[13]

My entrance into the Imperial Academy of Sciences at St. Petersburg as successor to Fr. Beilstein (d. 1906) led to my first visit to the United States, where I came as delegate to the VIIIth International Congress of Applied Chemistry (New York and Washington, 1912). Here, I made the acquaintance of Morley, Hudson, E. C. Franklin, Gomberg, Cottrell, A. A. Noyes, Langmuir, etc. I had already met H. C. Jones of Johns Hopkins in Europe.[14] The technical progress of the American assembly line struck me as especially wonderful. When (1912) we foreigners visited the Ford works at Detroit we were amazed at the weekly production rate: 6 automobiles! The cars at that time were quite asthmatic and unreliable; at the Chicago Exposition we had to get out and push! At the final session of the Congress I was elected president of the IXth congress to be held in St. Petersburg in 1915. However, World War I ruined this plan. The same fate overtook the building plans for a new spacious Institute for Scientific Research, which had been worked out by Prince Galitzin for the Academy of Physics, Karpinski for the Academy of Geology, and myself as representative of chemistry.

The Riga Hochschule went into exile and for three years (1915–18) I continued my activities as professor and rector at Moscow, undisturbed even by the Bolshevik revolution of 1917. After overcoming much opposition, permission was granted for us to return

[10] See WALDEN, P., *Bull. soc. chim. France*, [4] **47**, 36 (1930); JAMES, J. C., *J. Chem. Soc.*, **153**, 1095 (1950).

[11] See, *e. g.*, WALDEN, P., *Z. physik. Chem.*. **147**, 1 (1930); *Ber.*, **75**, 1891 (1942).

[12] WALDEN, P., *Kolloid-Z.*, **27**, 97 (1920).

[13] Incidentally, it may be noted that in 1907 I was offered the professorship vacated by Mendeleev at the University of St. Petersburg. I similarly declined offers from Graz as successor to Skraup, from Amsterdam where the post had been held by van't Hoff and Roozeboom, and from Breslau, as successor to Ladenburg.

[14] In Cleveland a reporter interviewed me about the next great chemical discovery. Simply to say something, I replied: "The matter of eggs." Several days later, the headlines stated: "Chemists make eggs." However, up to now, hens are still doing this better.

home, and in the summer of 1918 I moved the Hochschule back to Riga in cattle cars, where, however, we found a national revolution in progress.[15] I had had more than my fill of revolutions; in the summer of 1919 I fled to North Germany, and there found a new home.

From 1919 on, I was head of the chemistry department at the University of Rostock, and resumed my teaching and research. A pleasant interlude was provided by the invitation to come to Cornell as nonresident lecturer. Here (1927/28) I became acquainted with Dennis, Bancroft, and Trevor (the latter two had studied with Ostwald). Various lecture tours gave me new impressions of the tremendous technical and scientific potential and progress of the U. S. A. I lectured in Richmond, Columbus, Harvard (where I was the guest of T. W. Richards and made the acquaintance of the young Conant), Ann Arbor (where I again saw Gomberg), Providence (Kraus), Princeton (eminent pupils of Bodenstein), Yale, and New York (Columbia and Rockefeller Institute (Levene)). I later had the pleasure of counting Nichols (of Cornell) and Audrieth (Illinois) among my students.

In 1928, I took part in the convention at the Solvay Institute in Brussells and met many distinguished chemists and physicists. I delivered an invited lecture before the Societé chimique de France in 1928, and in 1934, as honorary president, I attended the IXth International Congress of Applied Chemistry at Madrid. I there renewed my acquaintance with such chemical luminaries as H. E. Armstrong, Le Chatelier, Barger, Jorrissen, Fourneau, Karrer, Delepine, G. N. Lewis, R. Robinson, Lowry, Parravano. This same year we celebrated the centenary of Mendeleev's birth, and welcomed to Leningrad the German chemical leaders, as well as Bruni (Italy), Svedberg and Palmaar (Sweden), and Heyrovsky (Prague). As "honorary guest," I attended the Xth International Congress at Rome in 1938; the political tension made itself felt and there was a noticeable coolness toward me on the part of certain old acquaintances.

Rostock was spared the horrors of World War II for several years but was bombed eventually. In the night of April 24/25, 1942, my home and its entire contents, including my chemical library of 10,000 volumes, were consumed by fire. Homeless and at my wit's end, I started southward the next day, living from place to place as best I could, until I finally found permanent lodgings in Tübingen. Here, I am now attached to the university under the title of "guest professor."

[15] Early in 1918, the Baltic barons invited the Germans to occupy Latvia. The Bolshevik troops were compelled to retire, and the Germans invaded the country and were allowed to remain as an army of occupation even after the Armistice, because the Allies were not prepared to guard the country against the Bolsheviks. However, independence was declared on November 18, 1918. The Russians at once invaded the new state and captured Riga early in January, 1919, but were driven out of the city in May. The country was not entirely cleared of foreign troops until 1920. (*Translator's note*)

WRITINGS AND HONORS

In addition to my teaching and research activities I contributed extensively to the chemical literature. My papers number in the hundreds and deal with factual, theoretical, historical, and biographical topics. The latter include extensive essays on Arrhenius, Berthelot, Berzelius, Glauber, van't Hoff, T. Lowitz, Mendeleev, Lothar Meyer, Wilhelm Ostwald, Paracelsus, Pasteur, and Ramsay. My books are: "Handbuch der Stereochemie" (with C. A. Bischoff) (1893); "Wilhelm Ostwald" (1903); "Die Lösungsmittel in geschichtlichen Aufeinanderfolge" (1910); "History of Chemistry in Russia" (in Russian) (1917); "Optische Umkehrerscheinungen" (Walden Inversion) (1919); "Molekulargrössen von Elektrolyten in nichtwässrigen Lösungen" (1923); "Elektrische Leitfähigkeit von Lösungen" (3 parts) (1923/4); "Elektrochemie nichtwässerigen Lösungen" (1924); "Chemie der freien Radikale" (1924); "Salts, Acids and Bases" (Cornell Lectures) (1929); "Mass, Zahl und Gewicht in der Chemie der Vergangenheit" (1931); "Goethe als Chemiker und Techniker" (1932, reprinted 1943); "Goethe und die Naturwissenschaften" (1933); "Geschichte der organischen Chemie seit 1880" (1941); "Drei Jahrtausende Chemie" (1944); "Geschichte der Chemie" (1947, 2nd ed. 1950). I edited the Ostwald-Drucker-Walden: "Handbuch der allgemeinen Chemie," Vols. 1–8 (1919–30); contributed extensively to Bugge's "Das Buch der grossen Chemiker" and to "Das Handwörterbuch der Naturwissenschaften," Landolt-Börnstein, "Physikalisch-chemische Tabellen."

My honors have been many and varied. In addition to orders and titles of distinction (including hereditary nobility) the Imperial Russian government recognized my services by a permanent seat in the Academy of Sciences at St. Petersburg, in succession to Fr. Beilstein. Honorary doctorates were conferred by Stuttgart (Dr.Ing.), Rostock (M.D.), and Madrid (Sc.D.). The Chemical Society of London, the Academy of Science in Leningrad, the Finnish Academy in Helsingfors, and numerous learned societies in Germany include my name in their list of honorary members. I am corresponding member of the French, Swedish, and Göttingen academies. I hold the Goethe Medal of the German realm and the Goethe Plaque of the city of Frankfurt, as well as other medals.

It is indeed a sad commentary on the present economic state of the world and a somewhat satirical presentation of the great social problem of caring for the aged, that instead of enjoying the vaunted "joys of old age" and "the dignified leisure" of which Cicero wrote, I, at the age of 87, and without the pension which had been assured me by two "cultured" states for my 50 years of teaching, now must keep on teaching in order to supplement the inadequate old-age allotment provided by the government.

DIVISION OF CHEMICAL EDUCATION OF THE AMERICAN CHEMICAL SOCIETY

IT'S YOUR DIVISION

THE Division of Chemical Education fills a unique place in the American Chemical Society for, without it and those who are or should be members of it, there could be no members in the other divisions either. The Division becomes strong and justifies its existence in so far as it meets and treats the common problems of the large group of teachers of chemistry in the A. C. S. That it has been relatively successful is shown by its burgeoning growth in recent years (from a membership of 250 in 1945 to 700 in 1950). With this growth come somewhat different duties. The smaller group had little difficulty in communication. Most of the members knew each other and exchange of ideas proceeded readily. But it is somewhat more difficult to keep 700 members informed of Divisional programs and to obtain from them the ideas essential to a continuing service of the Division to its members and to the Society as a whole. In an attempt to find some solution, partial though it may be, your Chairman has undertaken the job of columnist with the excellent cooperation of the Editor of the JOURNAL OF CHEMICAL EDUCATION who has given the space. We thus hope to keep the members informed as to current projects within the Division and to present for their comments suggestions as to new avenues of growth and service.

DIVISIONAL HISTORY

Each member of the Division will want to watch for the appearance of the History of the Division of Chemical Education which is to be printed in *Chemical and Engineering News* sometime between February and June, 1951. Dr. Harrison Hale of the University of Arkansas has gathered, for this series, the background material which will enable each of us to see where the Division has been, and what it has done, and from these to predict some possible courses for the future.

GRADUATE STUDENTS

With the new year begins the double job of writing letters of reference for students who will graduate in June and the selection, on the basis of these letters, of those who will begin graduate work in each school. In recent years many students have been placed in a quandary when they received an early acceptance from one graduate school, whereas their first choice did not notify them for another month or two. Similarly the graduate schools find that some of the early acceptances are later returned when a more attractive offer reaches the student at a later date. At one time there was a fairly regular procedure established for sending out notices of acceptance to graduate school at a uniform time and the Division now has a Committee under the chairmanship of R. C. Anderson of the University of Texas investigating the possibilities of a code to cover the hiring of graduate students and their acceptance. The other members of the Committee are James F. Corwin (Antioch), John E. Willard (Wisconsin), A. W. Laubengayer (Cornell), and your Chairman (Oberlin). Suggestions will be welcomed.

AND WHAT BECOMES OF THEM?

One of the hard parts of writing letters of reference which succeed in placing a student in a position is the subsequent difficulty in keeping track of the student and his progress. Dean Frank Smith of the Graduate School at Rochester has applied one of the best ideas we have seen in some time by writing to each of the men who served as references and letting them know when their candidate has achieved his degree. It seems to me that this might well be taken up by all graduate schools and industrial personnel departments as a courtesy to the men who originally supplied them with the information which led them to hire the student. Such a plan would certainly widen what should be a two-way street of communication between the educator and the user of his product.

J. A. CAMPBELL, *Chairman*

ERRATUM

An error in the paper by H. R. Davidson entitled "Dipole moments and molecular structure," appearing on page 598 of our November, 1950, issue, has been pointed out.

Equation (3) should be

$$P = \frac{M_2}{\rho_1} \times \frac{3}{(\epsilon_1 + 2)^2} S + \frac{\epsilon_1 - 1}{\epsilon_1 + 2} \times \frac{M_2}{\rho_2}$$

where S is the slope of the plot of dielectric constant versus weight proportion of solute.

Proceedings of the

PACIFIC SOUTHWEST ASSOCIATION OF CHEMISTRY TEACHERS

THE SEQUENCE OF TOPICS IN A BEGINNER'S COURSE[1]

KAROL J. MYSELS and CHARLES S. COPELAND
University of Southern California, Los Angeles 7, California

A COURSE, particularly a beginner's course in chemistry, reflects both the required or traditional curriculum and the point of view of the instructor. The over-all content of such a course is rather well defined and generally accepted, so that the point of view of the instructor is reflected mainly in the sequence in which topics are presented and in his approach to each topic. In order to satisfy certain aims of our own teaching, we have developed in the last few years a sequence which differs from any that we have seen, and the present article summarizes this organization. We realize that many instructors will find it unsuitable, but we hope that it will be helpful to those whose aims are similar to ours.

Our aims may perhaps be briefly summarized as follows. In teaching a beginner's course, or any other course for that matter, we feel that a clear distinction should be maintained between what is and what is not directly observable by means within the student's grasp. The former should be the subject of an authoritative statement and, if possible, of a demonstration. On the other hand, conclusions, inductions, hypotheses etc., should be given for what they are and, in particular, the reasoning leading to them should be clearly pointed out. The ultimate goal would be for the student not to depend on the instructor's word except for a limited number of statements of fact. To approach this goal it is necessary to proceed from the simple and familiar to the more complicated and less familiar. This tends to give the student a continuous grasp of the material studied and allows him to follow the reasoning better at each step. There is also a gradual extension of the range ofphenomena directly observable by means within the student's grasp. In other words, a good basis should be firmly laid at each point before proceeding to the next. Finally, we feel that related phenomena should be grouped together, both for ease of learning and as an illustration of the generality of the principles involved.

The following is the sequence of topics which we finally adopted.

I. *Physical Changes*
  (*a*) *Substance*
    Physical properties
    Mixtures, homogeneous and heterogeneous
    Freezing
    Fractionation, substance
    Equilibrium, Le Chatelier's Principle
    P-T diagrams, allotropes
    Solubility
    T-C diagrams, alloys
    Gases (empirical)
    Vapor-liquid equilibrium
    Distillation
  (*b*) *Molecule*
    Kinetic-molecular theory
    Distribution of velocities, equipartition
    Avogadro's Principle, M. W. of gases
    Van der Waals forces, condensation
    Dynamic equilibrium
    Activation energy
    M. W. of solutes, colligative properties

II. *Chemical Changes*
  $2H_2 + O_2 \rightarrow 2\,H_2O$
  Concept of reaction
  Atoms, A. W., formulas
  Equations, stoichiometry
  $H_2 + I_2 \leftrightarrows 2HI$, chemical equilibrium
  Rate, activation energy
  $N_2 + 3H_2 \leftrightarrows 2NH_3$, effect of all conditions
  $CaCO_3 \leftrightarrows CaO + CO_2$ heterogeneous equilibrium
  Reaction of O_2 with elements and compounds
  Oxidation, reduction, affinity, displacement

III. *Electrolytes*
  Electrolysis, ions
  Faraday's law, Millikan's experiment
  Cells, E. M. F., E°
  E. M. F. series, redox reactions
  Precipitation and dissolving
  Acids, bases, pH, amphoterism
  Hydration, complex ions
  Redox reactions of S and N

[1] Presented before the Division of Chemical Education during the 118th Meeting of the American Chemical Society, Chicago, Illinois, September, 1950, and before the second annual meeting of the PSACT in Los Angeles, November, 1950.

IV. *Atomic Structure and Bonding*
- Periodic table
- Natural radioactivity
- α-Particle scattering, nucleus
- Isotopes
- Nuclear reactions, tracers
- Diffraction: light, X-rays, electrons
- NaCl, I_2 crystal structures
- Ionic, covalent, semipolar bonds
- Electronic structure
- Survey of structures: crystals, molecules
- Resonance: graphite, Na

V. *Organic Chemistry*
- Hydrocarbons, isomerism, unsaturation
- Functional groups: OH, CHO, CO, COOH, NH_2, NO_2, SO_3
- Interaction of groups: ethers, esters, sugars, peptides

VI. *Colloid Chemistry*
- Nylon, condensation
- Lucite, polymerization
- Bakelite, crosslinking
- Rubber, vulcanization
- Silicones
- Starch, cellulose, proteins
- Hydrophilic groups, films
- Emulsions, soap, micelles, gels
- Adsorption, sols
- Ion exchange

VII. *Applications*
- Water treatment
- Fats and oils, paints, detergents
- Petroleum: drilling, refining
- Carbon cycle, wood, coal
- Nitrogen cycle
- Metallurgy: ore dressing, pretreatment, reduction, refining, alloying
- Industrial production of elements, bases, acids, salts

It will be noted that we omit all discussions centered around an element. The various elements and their compounds are brought in, however, when needed for the discussion of any topic. These instances are so numerous that they cannot be shown in the list, but descriptive chemistry must constantly be used to give concrete meaning to any discussion. For example, when we speak of allotropes, we treat together sulfur, tin, carbon, and the seven ices, and when discussing metallurgy we talk in close succession of iron, copper, tin, chromium, mercury, aluminum, magnesium, etc.

The concept of reaction is certainly a fundamental one, and is generally introduced very early. In our sequence it is introduced relatively late, in order to give it a proper foundation. For, isn't a reaction a process in which new substances and new molecules appear and old ones disappear? Unless, therefore, the student has a good grasp of the concepts of substance and of molecule, unless he knows how to tell whether a material is a substance or not, and how to distinguish between molecules, for example, by their molecular weights, he will not have a solid basis for understanding what a reaction is and will be just learning dogmatic words. However, the concepts of substance and of molecule do not require the concept of reaction for their development and can be established quite fully from such "physical" evidences as melting and boiling, fractionation by freezing or distillation, the properties of gases, and the kinetic molecular theory. In this sense, the concepts of substance and of molecule are simpler than that of reaction and are basic to it. They should, therefore, be developed first.

The concept of reaction leads, of course, directly to that of atoms and atomic weight. Atoms come to our attention mostly in the form of molecules, but the concept of a molecule can, as we have seen, be developed without recourse to atoms. The atomic weight cannot be deduced from purely chemical evidence which, at best, gives the combining weight. Nor can it be obtained directly without considerable speculation from physical evidence, such as mass spectrograph data which deal with ions, not atoms. The atomic weight can, however, be readily obtained if molecular weights are known, either by showing from combining volumes that the common elemental gases are at least diatomic or by considering the smallest weight of an element contained in the molecules of its compounds. Therefore, molecular weight logically precedes atomic weight. Molecular weight can be readily measured in the case of gases on the basis of Avogadro's Principle. For most substances one needs, however, some other method, and the colligative properties seem to be the most suitable for nonelectrolytes. Thus, we treat molecules, molecular weights, and colligative properties before reactions, atoms, and atomic weights.

A concept such as equilibrium, which is fundamental to almost all considerations, should be introduced early and in connection with examples which are as simple as possible. Such examples are obviously not chemical reactions, which in themselves are unfamiliar, but the simple freezing of water, solubility, vaporization, and so on. Once equilibrium and Le Chatelier's Principle have been solidly established for these phase equilibria, they can be extended to chemical systems, both homogeneous and heterogeneous. The concept of equilibrium is emphasized throughout our sequence although this is not shown in the list.

Similarly, the concepts of dynamic equilibrium, and of activation energy can be developed when vapor-liquid equilibrium is discussed in terms of molecules. Later they are readily applied to chemical processes.

We have found that if the concepts of substance, molecule, molecular weight, dynamic equilibrium, and Le Chatelier's Principle are firmly established, the discussion of reaction stoichiometry and chemical equilibrium is very easy on the basis of a few typical examples. All the basic "physical" material required may be found in most freshman courses, but scattered throughout the year. By bringing it together into the first ten weeks, we have not subtracted much from the total time devoted to "chemistry" but rather have consolidated the latter and made the treatment easier.

The same approach applies to more complicated topics. For example, the periodic table is generally used to show the regularity of properties of elements. If it is given early, before the student is familiar with many of these similarities, it becomes just a dogmatic

tabulation. If, on the contrary, the student is already familiar with many facts, it becomes a nice way of systematizing much of this knowledge and a good basis for going further.

When we come to atomic structure, we have to talk about atoms in terms of electrons and nuclei. These concepts should therefore have been studied earlier so that the student can use them freely. They can be well developed, however, without information about atomic structure. Thus, the concept of the atom comes from the study of molecules and reactions, the concept of the electron from the study of electricity and of electrolytes, and the concept of the nucleus from the study of radioactivity.

The first-year program generally includes not only the fundamentals of structure and behavior of matter, but also some information about industrial processes and everyday applications. Most of these turn out to be highly complicated, sometimes little understood, and generally involve a great variety of principles in every operation.

Thus, for example, the biosynthesis of proteins is not at all understood. On the other hand, water softening, while relatively well understood, involves such a multitude of factors as the solubility of electrolytes, solubility of gases, weak acid-base equilibrium, complex-ion formation, ion exchange, silicate and polymer structure, soap and other detergents. In order to discuss it intelligently and to answer fully the questions of students, who often know a great deal of what is actually done, these basic concepts should be well established beforehand. And the same is true of most other processes. Therefore, with a few exceptions, we feel that the serious study of applications should come after the general groundwork is given, although some applications may be thrown in earlier as spicing.

These few examples illustrate the kind of considerations which guided us in deciding on a sequence of topics. We feel that by this organization we can cover the topics normally treated and yet lead the student from the simple to the complicated and give him a solid basis for each successive step, thereby avoiding in large measure dogmatic statements of anything but fact.

MILLS COLLEGE MEETING

The Northern California Section of the PSACT held a meeting at Mills College on October 21, 1950.

Three speakers took part in a symposium on the subject Analytical Chemistry—How Should It Be Taught?

R. W. Bremmer of Fresno State College outlined the historical development of analytical chemistry. He considered the qualities and abilities that industry finds desirable in analytical chemists, and described the courses being given at Fresno State College to satisfy these needs, and also to satisfactorily care for the premedical and nonprofessional students. He spoke of the continued development of the courses to meet these various types of students.

John H. Wise of Stanford University described Stanford's curriculum in the field of analytical chemistry and the several courses developed to satisfy the various needs. He gave particular emphasis to recent changes in the content of several of the courses designed to prepare the professional student more satisfactorily (chemist, chemical engineer, premedical, or mineralogical student) for his life work.

Louis Lykken of Shell Development spoke from the industrial viewpoint. In most respects he agreed with the other speakers on the symposium. However, he emphasized several points such as the need for experienced laboratory teachers, and the fact that safety should be stressed more than it is. He outlined types and amounts of work that he thought would be desirable in the training of an industrial chemist.

AWARD IN CHEMICAL EDUCATION

NOMINATIONS for the Scientific Apparatus Makers Award in Chemical Education must be in the hands of the Secretary of the American Chemical Society (1155 Sixteenth St., N. W., Washington 6, D. C.) by June 1.

Any member of the Society may submit one nomination, which must be accompanied by eight copies of: a biographical sketch of the nominee, a list of his publications, a specific identification of the work on which the nomination is based, and an evaluation and appraisal of the nominee's accomplishments to be recognized in the proposed award.

A nominee must have made outstanding contributions to chemical education considered in its broadest meaning, including the training of professional chemists; the dissemination of reliable information about chemistry to prospective chemists, to members of the profession, to students in other fields, and to the general public; and the integration of chemistry into our educational system. Such activities may lie in the fields of teaching (at any level), organization and administration, influential writing, educational research, the methodology of instruction, establishment of standards of instruction, and public enlightenment. Preference is given to U. S. citizens.

Report of the

NEW ENGLAND ASSOCIATION OF CHEMISTRY TEACHERS

CHEMICAL PROBLEMS ENCOUNTERED IN FREEZING FOODS[1]

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Food and Container Institute for the Armed Forces, Research and Development Branch, Office of the Quartermaster General, Chicago, Illinois

THE changes which may occur during the preparation of food for freezing and during the subsequent freezing, storage, and thawing of the prepared food are physical, chemical, and physicochemical. Physical changes include crystallization, expansion, and desiccation. Chemical changes may be classified as (*a*) ordinary chemical actions, (*b*) those caused by enzymes, and (*c*) those brought about by microorganisms (bacteria, yeasts, and molds).

Chemical changes are of many types and usually occur simultaneously with each of the other two types of changes. Oxidation of pigments and catecholtannins, hydrolysis of fats and other esters, and dehydration or denaturation of proteins are examples of chemical changes which often occur in frozen foods.

Both fruits and vegetables are composed of living matter and consequently respire. That is to say, after harvest they continue to take up oxygen and give off carbon dioxide. This action causes a decrease in sugar content which is especially noticeable in peas and corn. Bruising and freezing both cause mixing of the cell contents and an abnormal respiration which may bring about the development of off-flavors. During vining (threshing) both peas and lima beans are severely bruised. This bruising mixes the cell contents and causes rapid "abnormal" enzyme actions which cause the quick development of off-flavors in the vined vegetables unless they are promptly heated to inactivate enzymes (blanched) or chilled.

Commercially, vegetables are blanched preparatory to freezing. This treatment kills the tissues and inactivates catalase and a considerable portion of the other enzymes so that enzyme actions, and consequently respiration, are very greatly reduced during freezing, cold storage, and thawing. As yet no one has shown what enzymes are responsible for the loss of color and changes in flavor of unheated frozen vegetables during cold storage. Practical experimentation has demonstrated that it is necessary to heat the vegetables long enough to inactivate catalase completely and to destroy practically all of the peroxidase or else "hay flavors" will develop during storage at 0°F. or above.

Although heating in boiling water or steam is necessary to destroy enzymes and thus prevent the loss of color and change of flavor during freezing and subsequent storage, this treatment dissolves out an appreciable proportion of the water-soluble nutrients and flavors. The losses of ascorbic acid (vitamin C) and sugar are particularly notable. The longer the blanching the greater the losses; therefore it is of considerable importance to blanch vegetables just long enough to inactivate the enzymes. Steam-blanching leaches out less ascorbic acid and other water-soluble nutrients than does water-blanching, but unfortunately steam-blanching does not give as uniformly desirable results with leafy and some other vegetables.

Fruit usually is not heated; consequently, enzyme actions often give trouble during freezing and also during cold storage and thawing. Freezing and cold storage even at low temperatures do not inactivate any of the common enzymes; when returned to ordinary temperatures their activity is found be to unimpaired. Enzyme actions are markedly slowed down at cold storage temperatures; in general, the lower the temperature, the slower the rate of action.

Oxidative enzyme actions are particularly objectionable in cold-stored fruit, since these actions cause darkening and changes in flavor. Frozen peaches, apricots, apples, pears, and sweet cherries are particularly subject to oxidation by enzyme action. In some instances these fruits are heated with steam in order to inactivate oxidative enzymes prior to freezing. In

[1] Abstract of an address presented at the Twelfth Summer Conference of the New England Association of Chemistry Teachers, University of Connecticut, Storrs, Connecticut, August 25, 1950.

other processes sulfur dioxide, sulfites, or ascorbic acid are added to the fruit. Sulfur dioxide and sulfites impart an objectionable flavor to the fruit but ascorbic acid actually improves both its flavor and nutritive value; furthermore, the fruit will not become discolored or changed in flavor until almost all of the ascorbic acid is oxidized.

In the case of many fruits which are not so easily changed in color and flavor by oxidation, sugar or sugar sirup is commonly added to retard enzymatic action. The action of the sugar is not perfectly understood, but it surrounds the fruit with sirup or sweetened juice, thus preventing intimate contact of the fruit with air.

Both oxidative and lipolytic enzymes give trouble during the cold storage of fish. Oxidative enzymes are particularly troublesome in the case of fatty fish such as mackerel and salmon. Fish oils are highly unsaturated and readily combine with oxygen, becoming discolored and rancid. Frozen whole fish may be protected against oxidation by glazing. This is carried out by dipping the frozen fish in cold water, thus forming a protective film of ice (glaze) over the surface. During storage this ice film slowly sublimes; consequently it is necessary to repeat the glazing operation every two or three months.

Fish fillets are sometimes dipped in an ascorbic acid solution prior to freezing in order to coat the fish with an antioxidant and thus prevent oxidation. Glazing with an ascorbic acid solution is even more effective.

Proper packaging is also of great importance in retarding both chemical and enzymatic changes, especially oxidative changes. The relative humidity of cold storages is often low, because of the condensation of the moisture on the refrigeration coils (ammonia pipes). Consequently, unless the food is packaged in airtight or moistureproof packages, there is a slow movement of moisture from the product to the refrigeration coils. This loss of moisture from the surface of poultry, meat, and fish causes freezer burn. The loss of this protective film of moisture exposes the surface to oxidation which is often severe. If paper packages are used for frozen foods, it is necessary to make them impervious to moisture vapor, and, preferably, also to air. In some instances this is done by lining or covering them with moistureproof cellophane, pliofilm, or a plastic sheeting. In other packages this is accomplished by coating or lining them with a suitable wax or other plastic. For some products, *e. g.*, fruit juices, glass or metal packages are used.

Bacteria, yeasts, and molds are active at ordinary room temperatures, 70° to 80°F., multiplying rapidly and causing profound changes in both fruits and vegetables. As the temperature is lowered, the rate of growth of these organisms is rapidly reduced until, at about 15°F., their growth becomes negligible. Since frozen foods are ordinarily stored at temperatures below 15°F., the action of microorganisms is a hazard only before freezing and during and after thawing.

The physicochemical and colloidal changes occurring are probably equal in importance to the chemical changes. The changes in the colloidal condition of frozen foods are profound, but not well understood. Meat, poultry, fish, and shellfish are all made up of jelly-like protoplasm—a gel, to the colloid chemist. To fix the original spatial distribution of the colloid the rate of freezing must be rapid enough to form minute crystals uniformly distributed throughout the tissue. If such a quick-frozen product is thawed immediately, the moisture is reabsorbed as the crystals melt. If, on the other hand, the freezing is very slow, or the quick-frozen product is held under conditions which permit either the growth of the crystals or the irreversible dehydration or denaturation of some of the proteins, the product does not entirely return to its original gel condition. Some of the liquid resulting from the thawing of the crystals leaks out as "drip" or "leakage."

OFFICIAL BUSINESS

The 257th Meeting of the New England Association of Chemistry Teachers was held on Saturday, October 14, 1950, at the Nashua Senior High School, Nashua, New Hampshire. Approximately 50 persons attended the meeting.

Marco Scheer, Chairman of the Northern Division, presided. He introduced Edmund M. Keefe, Headmaster of the Nashua Senior High School, who brought greetings to those present and spoke briefly on the ways in which the senior high school is attempting to fit its program to the individual student.

The first paper of the morning was delivered by Geoffrey Broughton of the Lowell Textile Institute, who spoke on "Paper, an old product with an expanding future." Professor Broughton outlined briefly the history of the paper industry and then drew attention to the many new uses to which paper is being put. He brought with him a large number of samples to illustrate these various new uses.

The second speaker was William C. Tallman, Technical Assistant for the Public Service Company of New Hampshire, who spoke on the "Mercury vapor turbine," discussing the theory behind its operation and showing under what circumstances such a steam turbine would be used.

After the luncheon, opportunity was given for inspecting the school. Particularly interesting were the machine, wood-working, and automobile shops, as well as the laboratories for home economics, chemistry, biology, and physics.

A business meeting was called to order by the President, Helen W. Crawley. For the Membership Committee, the Secretary reported the election of the following new member:

Miss Rita P. Landry, Chemistry Teacher, Lowell High School, Lowell, Massachusetts.

Miss Crawley announced that she has been made a member of the Committee in charge of the Massa-

chusetts State Science Fair. This Committee has met with a representative from the *Boston Globe* in an effort to clear up the misunderstanding which arose last year because the fair was known as the *Boston Globe* Science Fair. The *Boston Globe* has pointed out that it is under an expense of several thousand dollars in sponsoring this fair, and that since it has assumed this financial responsibility it feels that it is only right that the name of the paper should be associated with the project. The *Globe* and the Committee have agreed, therefore, that the fair shall be called "The Massachusetts Science Fair, Sponsored by the *Boston Globe*." It is hoped that in this way science teachers will realize that the fair is definitely for the entire state and that the *Boston Globe* is acting simply to underwrite the expenses for it. There were other comments from the floor indicating appreciation of the willingness of the *Globe* to assume such a financial burden for the sake of holding the fair.

The last speaker of the day was Albert F. Daggett, Professor of Chemistry and Dean of the Graduate School at the University of New Hampshire. Dr. Daggett spoke on "Element 61." After a brief survey indicating the position occupied by element 61 in the periodic system, Dr. Daggett discussed the recent work which has been done at the University of New Hampshire in an effort to establish whether this element exists in nature or not. He discussed methods followed in the research work utilizing ion-exchange resins. He closed by stating that there is so far no conclusive proof of the existence of the element in nature, but at the same time it should be recognized that there is no proof of its non-existence. Work continues on the problem.

The 258th Meeting of the New England Association of Chemistry Teachers was held at Simmons College, Boston, Massachusetts, on December 2, 1950, as a joint meeting with the Eastern Association of Physics Teachers and the New England Biological Association. Ina Granara, Chairman of the Central Division, presided. This was an unusually large meeting, over 200 people attending. The room originally assigned could not hold the group and it was twice necessary to move to larger rooms.

The meeting was opened with greetings by John A. Timm who is Director of the School of Science at Simmons College.

Clarence C. Little of the Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine, spoke on "Cancer research and the secondary schools." Dr. Little's chief thesis was that in the present unsettled conditions it is particularly necessary to awaken the interest in research in the brilliant student while he is still young enough to have uninterrupted opportunity for doing something of this sort, and he pointed out that such an opportunity for research is being offered at the present time at the Jackson Memorial Laboratory.

I. Bernard Cohen of Harvard University spoke on "General education in the sciences." In a brilliant and inspirational address which deplored the present-day emphasis on the gadgets and literature of learning, he begged teachers to stress critical thinking. Through analogies drawn from various personalities, situations, and theories in the history of science, Professor Cohen developed his thesis that we must cease asking students to "learn and learn and learn," and that we must, instead, endeavor to train them to see what they really see, not what they think they should observe, and to think critically about the observations which they have made.

The last speaker of the morning was Royal M. Frye, Head of the Physics Department, Simmons College. Professor Frye gave a very clear review of "Physics in 1950," and in the time at his disposal elaborated on some of the more important points.

The afternoon session opened with an address by Hollis S. Baird of Northeastern University who spoke on "A review of television, past, present, and future." In addition to giving a very comprehensive survey of the problems and techniques of television with special emphasis on color television, Dr. Baird demonstrated projection television and made it possible for the audience to see the last few minutes of the Army-Navy game. This innovation was received with considerable enthusiasm.

Following Dr. Baird's lecture, President Helen W. Crawley called a short business meeting. The Treasurer reported that to date (December 2) there are 404 members who have paid their dues, and that of these, 328 have sent a 50-cent contribution for the *News Letter*. This makes the *News Letter* solvent for the year. In the absence of the secretary there was no report of the Membership Committee.

It was announced that the dates for the Massachusetts Science Fair have been set for May 4, 5, and 6, 1951.

SUMMER CONFERENCE COMMITTEE

The Thirteenth annual summer NEACT conference is planned for the week of August 19, 1951, and will be held on the campus of Rhode Island State College, Kingston, Rhode Island. President Helen W. Crawley has announced the appointment of the following committee:

Co-Chairmen
Donald C. Gregg, University of Vermont.
William S. Huber, Rhode Island School of Design.

Secretary
Elizabeth W. Sawyer, Connecticut College.

Registrar and Treasurer
Virginia W. Duval, Catonsville High School, Catonsville, Maryland.

Assistant Registrar
Dorothy Abel, Brookline High School, Brookline, Massachusetts.

Committee Chairmen

Representative of Host Institution: W. George Parks, Rhode Island State College, Kingston, Rhode Island.
Publicity: Rev. Leo J. Daily, St. Thomas Seminary, Bloomfield, Connecticut.
Exhibits: George Deckey, Rhode Island, School of Design.
Printing and Mailing: Leslie A. Hallock, W. M. Welch Scientific Company, Worcester, Massachusetts.
Motion Pictures: Ralph Di Mattia, Dorchester, Massachusetts.
Social Events: Ruth R. Jefferson, Branford, Connecticut.

Committee Members

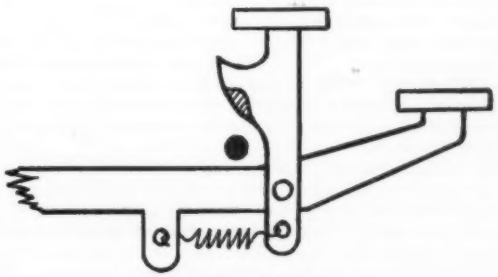
Guy F. Burrill, Keene, New Hampshire.
Angelica Courniotes, Chicopee Falls, Massachusetts.
Helen W. Crawley, NEACT President, *ex officio*, Walnut Hill School, Natick, Massachusetts.
Standish Deake, Woodstock Country School, Woodstock, Vermont.
Robert D. Eddy, Tufts College, Medford, Massachusetts.
Ruth H. Ellis, Vassar College, Poughkeepsie, New York.
Rose Finkelstein, Woodbury, Connecticut.
Clinton S. Johnson, Rhode Island College of Pharmacy, Providence, Rhode Island.
Bernard S. Jurale, Meriden, Connecticut.
Alfred R. Lincoln, Western Massachusetts College of Pharmacy, Willimansett, Massachusetts.
Russell Meinhold, Rhode Island College of Education, Providence, Rhode Island.
Katherine M. Murphy, Dorchester, Massachusetts.
Andrew J. O'Connell, Worcester Academy, Worcester, Massachusetts.
Marian Stine Sowers, Somerville, New Jersey.
Walter G. Stewart, Springfield, Massachusetts.
Louise O. C. Swenson, Lynn, Massachusetts.
Carl P. Swinnerton, Pomfret School, Pomfret, Connecticut.
Elbert C. Weaver, Phillips Academy, Andover, Massachusetts.
Roland M. Whittaker, Queens College, Flushing, New York.
Eugene C. Winslow, Rhode Island State College, Kingston, Rhode Island.

The summer conference program will be printed in the June, 1951, issue of THIS JOURNAL. The conference is open to all, members and non-members alike. Inquiries should be addressed to the Conference Secretary, Miss Elizabeth W. Sawyer, Scotland Road, Norwichtown, Connecticut.

To the Editor:

The article by A. H. Kunz (THIS JOURNAL, 27, 568 (Oct., 1950)) on the adaptation of a standard keyboard typewriter for use by a chemist prompts me to write concerning my own experience. Some dozen years ago, it was pointed out to me that if the shift key is only slightly depressed, the lower case letters and numbers will be written as subscripts to the normal line. If one wishes, one can make a special key and clamp it onto the frame so that it will depress the shift key only the proper distance. With such a device, one can write subscripts with no more effort than writing capitals. A chemical formula, such as $C_{12}H_{22}O_{11}$, or a special symbol such as P_s or b_x may be written directly and consecutively without resort to turning the platen. By using the special key depressed to make the standard line, superscripts will be written when the shift is in either of its normal positions. I have had little occasion to use this system, but it is capable of raising any lower case letter or number to any lower case letter, upper case letter, or number power.

When I looked over my own portable to make plans for such a key, I found that a new key was not required. All I had to do was file a notch at the proper spot on the under side of the shift lock. Now, by pressing the notched lock forward and then down, I can set the platen in the proper position for subscripts. By pressing it down and then forward, it operates normally



Shift Lock and Shift Key

The solid circle represents the pin fixed to the frame which locks the shift down when the shift lock key is caught under it. The cross-hatched portion represents the notch filed to stop the shift in a partially depressed position when it catches against the pin.

as a shift lock. I have no idea of the number of typewriters that can be similarly adapted, but if such a change can be made, I would recommend it highly.

Correspondence from the manufacturers advised me that this scheme is a very poor one, for the keys contact the platen at an angle when the shift is partially depressed. They predicted that my machine would be very quickly ruined, for their keys are designed to strike squarely against the platen. All I can say is that they make a stronger machine than they realize, for I have pounded mine steadily (and hard) for at least twelve years. The present indications are that it will be corrosion by chemical vapors and not erosion by chemical formulas which eventually puts my machine in the discard.

ROBERT D. EDDY

TUFTS COLLEGE
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Recent Books

● COLLEGE CHEMISTRY

Linus Pauling, **Professor of Chemistry in the California Institute of Technology. W. H. Freeman and Company, San Francisco, 1950. x + 705 pp. 213 figs. 37 tables. 16 × 24 cm. $4.50.**

THOSE who have used Pauling's "General Chemistry" will welcome this new book because it fulfills very well the purpose of the author to furnish a first-year chemistry text "written in a more slowly paced, less mathematical form." Although the entire book has been reset in a more easily read type, and new and improved illustrations have been added, much of the older book remains.

The most radical change has been the treatment of gases and the gas laws. These subjects are considered much earlier than in "General Chemistry," and much discussion is devoted to the physical behavior of gases before the gas laws are presented in detail. The perfect gas equation is deferred until after a thorough treatment of Boyle's and Charles' laws. Discussion of van der Waals' equation has been deleted.

Other outstanding changes include a chapter devoted to the chemistry of the common elements, introduction to biochemistry and more detailed treatment of photography, including color photography. Chromium and manganese are considered with the other transitional metals in a chapter much nearer the end of the book. The principle of Le Chatelier has been shifted to the chapter on water and is illustrated by reactions involving water softening by a zeolite. The equilibrium between nitrogen dioxide and dinitrogen tetroxide is also used to discuss this important principle in a combined chapter on chemical equilibrium and the rate of chemical reaction; it is further discussed and illustrated by heat of solution.

The discussion of molar, normal, and molal solutions has receded to the conventional, and it is with a twinge of lament that one reads the footnote on page 340: "A few authors have used molality as the moles per liter of solution, but this usage has not been accepted." The term "formality," has been retained to express the number of gram formula weights per liter of solution. More confusion is likely to result from the use of "*molar* freezing point constant" ("weight-molar, moles per 1000 g. of solvent"), on page 349, than the use of molal freezing point constant in the earlier book, even though, for Pauling, molal had a different meaning. Pauling addicts will find that cyanuric triazide is notably absent.

A discrepancy carried over from the earlier book appears in discussions of the diameter and radius, respectively, of the electron. The *diameter* (page 35) is given as 10^{-12} cm., while the *radius* is correctly stated (page 71) to be "about 1×10^{-12} cm." Again on page 575 gasoline is stated to be a "heptane-to-nonane mixture" while on page 579 appears "...normal hexane (gasoline)....." Another misprint, is this statement (page 50): "The randomness of structure usually causes the density of a liquid to be somewhat less than that of the corresponding crystal; that is, the volume occupied by the liquid is usually somewhat *less* than that occupied by the crystal." Misprints, are however, infrequent and most of the rare misprints in the earlier book have been corrected in this one.

This is one of the first elementary texts to list and discuss elements 97 and 98, and to incorporate the decisions of the September, 1949, meeting of the International Union of Pure and Applied Chemistry. The hydrogen bomb is discussed in a subchapter relating nuclear fission and nuclear fusion. The name of Celsius has been mentioned with the Centigrade thermometer scale, but the latter term is not discarded.

A conscientious effort has been made to clarify many of the more difficult passages which puzzled students in the first text, but curiously enough this gem of clarity has been retained (page 347): "At constant temperature, the partial pressure in the gas phase of one component of a solution is, at equilibrium, proportional to the concentration of the component in the solution, in the region of low concentration." Fortunately, Pauling's own translation follows: "This is equivalent to saying that the solubility of a gas in a liquid is proportional to the partial pressure of the gas." (Henry's Law).

Pauling's new book will not be easy reading for many college freshmen. Nevertheless, many students of only average ability in chemistry will find the book extremely interesting, and instructors will find that, in many cases, the teaching from Pauling will not be more difficult than that from other, less exacting, texts. Dr. Pauling's new book should prove very successful in any course on the freshman level in which quality of concepts is important. In any case, a student using this book will receive an excellent introduction to the "science of substances," and be well prepared for advancement in later science courses.

The impact of the Pauling texts on college chemistry teaching will be felt for a long time. It is with much interest that we now look forward to the rewriting of the more advanced "General Chemistry."

JOHN M. FLOWERS, JR.

THE COLLEGE OF WILLIAM AND MARY AND
THE VIRGINIA POLYTECHNIC INSTITUTE
NORFOLK 8, VIRGINIA

● MODERN GLASS WORKING AND LABORATORY TECHNIQUE

M. C. Nokes. **Chemical Publishing Co., Inc., Brooklyn, New York, 1950. xiii + 157 pp. 96 figs. 13.5 × 20.5 cm. $3.75.**

THIS is a valuable little book, especially for teachers who find it necessary to do their own laboratory constructional jobs (and who doesn't?). Not only does it explain and illustrate the principal types of glass-working operations—for soft glass as well as borosilicate—but it also deals with such topics as sealing metals into glass, electric welding, vacuum tubes, photoelectric tubes, and vacuum technique.

PHYSICAL CHEMISTRY FOR PREMEDICAL STUDENTS

John Page Amsden, Professor of Chemistry, Dartmouth College. Second Edition. McGraw-Hill Book Company, Inc., New York, 1950. xi + 317 pp. 54 figs. 38 tables. 16 × 23 cm. $4.25.

THE first edition of this book [reviewed in THIS JOURNAL, 23, 362 (July, 1946)] appeared four years ago, designed as a text for a one-semester course primarily for students who hope to study medicine. In the new edition, the plan is much the same as in the old.

The obvious changes are two in number: the addition of a chapter on thermodynamics, and a complete rewriting of certain material, formerly classed under a variety of headings, in a single unified treatment of protolytic reactions. Otherwise, the principal changes involve omissions of some material which experience has shown to be of comparatively little value to the student, clarification of expression in several places, and correction of errors and misleading statements which appeared in the first edition. Answers to the problems at the ends of the chapters have been omitted from this edition.

Whether or not it is wise to attempt to introduce thermodynamics formally into a course of this sort is a matter of opinion. Certainly, very little time at best can be devoted to the subject in such a course, and the student will not usually have the mathematical background ordinarily considered necessary for this subject. Given these limitations, the treatment presented here is quite satisfactory. Since later chapters are written so that they can be understood even without this material, its inclusion does no harm and will doubtless be helpful to those teachers who feel (as at least a few do) that some exposure to thermodynamics is desirable even in the brief course for which this book is intended.

The other major change, in the treatment of protolysis, meets with this reviewer's hearty approval. In the earlier edition, as in many similar books, equilibria involving proton transfers were discussed under a variety of heads, such as ionization of weak acids, weak bases, and water, hydrolysis of salts, etc. Here the various reactions of this sort are considered under a single heading. This unification becomes particularly helpful when quantitative aspects of the subject are discussed, because the various "constants" are clearly shown to be special cases of a single general principle. Unless the student has been thoroughly confused by less enlightened teaching in his earlier courses he should be able to handle this important material without much difficulty when it is presented in this manner.

WILLIAM E. CADBURY, JR.

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HAVERFORD, PENNSYLVANIA

METHODS OF QUANTITATIVE MICRO-ANALYSIS

Edited by *R. F. Milton,* Consultant Analytical and Biochemist, Welbeck Way, London, and *W. A. Waters,* Fellow of Balliol College, Oxford; University Lecturer in Organic Chemistry, Oxford University. Longmans, Green & Co., Inc., New York, 1949. viii + 599 pp. 169 figs. 21 tables. 15.5 × 23.5 cm. $15.

CONTRIBUTORS to this volume include not only the editors but also G. Ingram, Microanalyst, Courtaulds' Fundamental Research Laboratories; J. T. Stock, Norwood Technical Institute; K. M. Wilson, Physiological Chemist, Ministry of Supply.

The book should be read in the light of the editors' observation in the preface that: "very few, if indeed any, micro-chemical techniques are as yet generally taught to the average chemistry student, and most unfortunately it has happened that many of the extremely valuable micro-analytical procedures have been developed by such extreme specialists that even the average professional analyst is not even now fully conversant with them or aware of their wide potentialities." It is intended to supplement the standard works on analytical chemistry and to describe and review the application of standard analytical procedures to the microchemical field. In this it is very successful and the frequent tabular summaries of methods, procedures, etc., are among its most valuable features.

The book is divided into six parts, devoted respectively to: gravimetric apparatus and general microchemical techniques; microanalysis of organic compounds; volumetric analysis; colorimetric analysis; electrochemical methods; gasometric methods. It is probably too much to expect a book of this kind to cover exhaustively the many specialties in such a broad field, even with the combined efforts of five collaborators.

There are very few references to work published within the last five years, during which time there has been considerable progress in this field. One finds no mention, for example, of Kirk's many contributions to microchemical techniques, or the important developments in ion exchange separations, a subject which is disposed of in half a page. Modern instruments are covered very imperfectly. No spectrophotometer is even mentioned, and one gets a very inadequate impression of the possibilities of photoelectric colorimeters, recording polarographs, and glass electrodes. The colorimetric determination of pH probably does not deserve to be disposed of quite so pessimistically.

The authors have tried to limit their discussion to those methods which they have personally verified, and this is probably the reason why some methods seem to be over-emphasized while others have been neglected. Nevertheless, the collection of such a wide variety of material, presumably evaluated critically, makes this a useful reference source, particularly for students of analytical chemistry.

NORRIS W. RAKESTRAW

SCRIPPS INSTITUTION OF OCEANOGRAPHY
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GAS PRODUCERS AND BLAST FURNACES

Wilhelm Gumz, Consultant, Battelle Memorial Institute, Columbus, Ohio. John Wiley and Sons, Inc., New York, 1950. xii + 316 pp. 14.5 × 22 cm. $7.

THIS book is not a descriptive text on the types and methods of operation of gas producers and blast furnaces. Rather, it is an important contribution to the stoichiometry and thermodynamics of equipment in which solid fuels are gasified, which may be followed by subsequent reactions, as in the blast furnace. Section I on gas producers comprises 172 pages, Section II on blast furnaces is 103 pages, Section III on reaction kinetics is 22 pages, and an appendix contains 11 pages of tabulated data. Because of his European background Dr. Gumz works entirely in the metric system. The symbols sometimes used may be unfamiliar in American practice.

As an example of the fundamental nature of this book, the second chapter deals with gas composition at equilibrium. Methods are given to calculate equilibrium composition for single reactions. This is then expanded for the general case of eight equations and unknowns, and methods of simplification for the solution are given.

This book will be useful to chemical engineers teaching stoichiometry or thermodynamics, or interested in gasification processes.

KENNETH A. KOBE

UNIVERSITY OF TEXAS
AUSTIN, TEXAS

ANTIHISTAMINES: INDUSTRY AND PRODUCT SURVEY

Nathan Wishnefsky. Chemonomics, Inc., New York, 1950. 157 pp. 15.5 × 23 cm. $5.

Although many readers will think that the important part of this little book is the section dealing with antihistamines as cold remedies, the book goes far beyond that. The chemistry and biological significance of histamine itself, as well as the properties, structure, and preparation of the many antihistamines, is covered in some detail.

● ORGANIC CHEMISTRY

Louis F. Fieser **and Mary** ***Fieser.*** **Second edition. D. C. Heath and Company, Boston, 1950. xv + 1125 pp. 16 × 23.5 cm. Text edition $7.50; trade edition, distributed by Reinhold, 330 West 42nd Street, New York, $10.**

THIS thoroughly revised second edition of Fieser and Fieser's distinguished text, "Organic Chemistry," has continued the trend of recent organic books to place greater emphasis on the electronic interpretation of organic reactions at the introductory level. The authors' stated aim has been to "modernize the book by application of electronic and resonance concepts wherever such theoretical interpretation is well grounded and helpful." The authors intend the book to be both "an introduction to organic chemistry and a general survey of applications of organic chemistry to biological and medical sciences and to technology."

In all, the book contains 42 chapters of which chapters 1–4, 6–12, 15–17, and 22–34 (approximately 700 of the 1100 pages) constitute an introductory course. The Fiesers point out that chapters 12, 15, 17, 30–34 contain more material than is needed for a first course and this reviewer heartily agrees with them. This second edition contains two completely new chapters, one on heterocycles (35 pages) and one on reaction mechanisms (25 pages). Also it contains a 3-page appendix giving bond distances and energies, resonance energies, and solubility data for a number of compounds. Electronegativity values for several elements are also given. The content of the second edition has been increased considerably by the addition of 34 more pages, plus the adoption of a format that affords 3 more lines per page, and by certain "techniques of composition." Although the general approach is the same as that of the first edition, the authors have kept in mind more clearly the dual function of the book, that of serving as an introduction to the beginner and that of acting as a reference book for more advanced students. They have accomplished this two fold objective admirably.

The organization of the material in the first 13 chapters is the same as in the first edition. However, considerable new material on interesting recent developments in organic chemistry has been added, for example, the oxo process, lithium aluminum hydride, nitrogen mustards, BAL, conversion of nitriles to amides by H_2O_2, and chemistry of β-propiolactone.

The platform is set for the discussion of electronic reaction mechanisms by a brief consideration of electronic structure of several elements in the first chapter. The initial consideration of a reaction mechanism in any detail is on page 44, for the free radical chlorination of hydrocarbons. The polarized double bond and coordinate covalent bond are considered on pages 52 and 53. The concept of the Lewis acids and bases is introduced in the chapter on alcohols (pages 139–40) and then again in the chapter on amines (pages 221–2). A two-page introduction is given to resonance in the chapter on carboxylic acids with consideration of the carboxylic group and the allylic rearrangement. The mechanism of esterification is considered briefly on pages 176–7. With this gradual but brief introduction to modern concepts of reaction mechanisms the student is introduced rather suddenly into the subject at page 343 in the chapter on Reaction Mechanisms. Here in a very tightly written chapter of 25 pages, the Fiesers present to the student the important fundamentals supporting the modern concepts of organic reaction mechanisms. Since a considerable amount of this space is taken up by tables, equations, and questions, many terms such as transition state, inductive effect, first order kinetics, nucleophilic and hyperconjugation are not treated in much detail. Only the exceptional student will be able to gain a good understanding of principles with which to interpret other organic reactions without considerable outside help. The Fiesers do not recommend this chapter, 14, for inclusion in an introductory course. Since the bulk of the discussion of reaction mechanisms (an excellent discussion of the mechanism of substitution and orientation in the benzene ring is given in chapter 24) is confined to this chapter, some instructors, along with this reviewer, may feel that insufficient emphasis has been placed on the subject of mechanisms.

In the second half of the book the Fiesers have been careful to include the many recent organic developments, such as: Calvin and Benson's work with radioactive CO_2 in photosynthesis, phthalyl synthesis of peptides, chemistry of cyclooctatetraene, Newman's work on esterification of *o*-substituted acids, new fibers such as Dynel, Orlon, and Terylene, new dyes such as Khaki 2G and Indocarbon CL, new material on cortical hormones, antimalarials, and the recent work on antibiotics.

Many of the chapters have been completely reorganized. For example, the chapter on steroids has been shortened in part by omitting some of the long syntheses and by shortening others. New material on the stereochemistry of the sterols has been included. The chapters on rubber and quinones have also been condensed.

The first half of the new chapter on heterocycles is spent in description of the different systems and a few of their basic reactions. The last half discusses the chemistry of the alkaloids, anthocyanines, flavones, and miscellaneous pigments.

This book is very nicely printed, well bound, and amazingly free from typographical errors. Fieser and Fieser will continue to hold its place as one of the outstanding organic texts in the English language.

CORWIN HANSCH

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CLAREMONT, CALIFORNIA

● SALES AND BUSINESS FORECASTING IN CHEMICAL PROCESS INDUSTRIES

Robert S. Aries **and** ***William Copulsky,*** **Adjunct Professor of Chemical Engineering, Polytechnic Institute of Brooklyn, and Research Economist, R. S. Aries and Associates, respectively. Chemonomics, Inc., New York, 1950. x + 132 pp. 17 × 26 cm. $5.**

THIS book presents the methods used in forecasting the change in economic factors in chemical industry. The ways in which these methods are used by a number of chemical companies and governmental agencies are shown.

Unfortunately, the book is lithoprinted from sheets typed with light Gothic type and is difficult to read.

KENNETH A. KOBE

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● EXPERIMENTS IN PHYSICAL CHEMISTRY

Otto F. Steinbach, **Assistant Professor of Chemistry, Queens College, Flushing, New York, and** ***Cecil V. King,*** **Professor of Chemistry, New York University, New York City. American Book Company, New York, 1950. vi + 250 pp. 71 figs. 45 tables. 14 × 21 cm. $3.50.**

THIS addition to the list of books dealing with the laboratory work of physical chemistry will be of interest especially to those teachers seeking an easily usable manual in that field. It gives brief but comprehensible directions for the performance of fifty experiments, each one of which would require the customary 2–3 hour period. Thus there are about 20 experiments in excess of the normal year's work and, in consequence, there is provision for considerable choice.

The write-up of each experiment includes a concise discussion of the theory involved, a straightforward description of the procedure, with illustrative diagrams in most cases, and directions regarding the tabulation and treatment of the results. References to original papers pertinent to the subject are given at the end of each experiment; it may be noted that recent publications have not been neglected.

The choice of experiments is largely conventional but the field is covered in a highly satisfactory manner. One may miss such favorites as the determination of the formula of the silver

diammine complex ion from freezing point measurements and of the rate of saponification of an ester by the conductance method. On the other hand, the inclusion of experiments on "Surface Active Substances," "Interfacial Tension," and activity coefficients, is to be commended. An introduction of 19 pages deals with errors, significant figures, graphical representation of data, and the use of thermometers, manometers, and certain other apparatus.

The book is compact and of neat format. The cover design is unusual—the reproduction of a photograph of part of a distillation system—which some might consider inappropriate on the jacket of a physical chemistry manual.

The authors are to be commended for the careful preparation of what seems to the reviewer to be an excellent manual.

WILLIAM B. MELDRUM

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• CHYMIA. VOLUME 3

Editor-in-Chief: *Henry M. Leicester.* University of Pennsylvania Press, Philadelphia, 1950. ix + 251 pp. 72 figs. 16 × 24 cm. $4.50.

THE international character of this historical annual is evident from the fact that Volume 3 contains articles in four languages—English, French, German, and Spanish. This in itself makes it almost unique among American publications.

Appropriately enough, the first article, by Henry M. Leicester and Herbert S. Klickstein, deals with the work of the late Tenney L. Davis, the first editor of "Chymia." It includes a complete bibliography of his contribution to the history of chemistry, which would please Dr. Davis, who insisted upon this feature as an accompaniment of every good biography.

The other outstanding articles deal with:

The beginning of chemical instruction in America. This is really an account of chemistry teaching at Harvard before 1800, by Bernard Cohen.

Lavoisier's experimental apparatus (in French), by Maurice Daumas.

Some early American chemical societies. The story of two chemical societies which held sway for a few years in Philadelphia at the beginning of the nineteenth century is told by Wyndham Miles.

Robert Boyle and Pierre Bayle. George Sarton deals with the lives of these two under the respective subheadings, "Sceptical Chemist" and "Sceptical Historian."

Henri Sainte-Claire Deville. The works of a neglected but brilliant French chemist are recorded by Ralph E. Oesper and Pierre Lemay.

Bunsen's "lecture" on general experimental chemistry (in German), by Heinrich Rheinboldt. This was in reality a course of about 100 lectures which Bunsen gave at Heidelberg for a period of 74 consecutive semesters. A study of his methods and his ingenuity in overcoming difficulties will be well rewarding to present-day teachers of this subject.

NORRIS W. RAKESTRAW

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• CELLULOSE ACETATE PLASTICS

***Vivian Stannett.* Temple Press, Ltd., Bowling Green Lane, London, E.C.I., 1950. xxiv + 325 pp. 197 figs. 57 tables. 14.5 × 22 cm. 30/-.**

THIS appears to be the first book devoted entirely to cellulose acetate plastics which, the author states, are the most versatile of all plastic materials in methods of application. Cellulose acetate plastics reached in 1946 a peak production in the U. S. A. of 103 million pounds, of which more than 80 per cent was used for molding and extrusion.

While the commercial manufacture of acetate rayon began in 1919, the utilization of cellulose acetate as a plastic did not find success until about 10 years later—after the development of suitable plasticizers for it. In the succeeding 20 years a wealth of information relating to this plastic has been made available in the technical literature.

The author of this volume exhibits thorough familiarity with both British and American processes and applications. His illustrations and references are drawn about equally from the two sides of the Atlantic. He has brought together a great amount of useful information, with an extensive bibliography, covering all aspects of cellulose acetate as a plastic. These include its manufacture, the production of film, sheet and molding powder, its fabrication and finishing, and the specific applications of its various forms. The chemical and the mechanical features of the subject are treated with apparently equal competence. This reviewer found the sections dealing with plasticizers and with the properties of cellulose acetate plastics particularly interesting and valuable.

This book is written in a clear, intelligible style and the printing, including the many illustrations, is excellent. It can be recommended as a thorough and authoritative treatment of the subject from the industrial viewpoint.

HARVEY A. NEVILLE

LEHIGH UNIVERSITY
BETHLEHEM, PENNSYLVANIA

• THE CHEMISTRY OF THE ACETYLENIC COMPOUNDS VOLUME II: THE ACETYLENIC ACIDS

***A. W. Johnson.* Longmans, Green and Co., New York, 1950. xxvii + 328 pp. 15 × 22 cm. $10.**

THIS is the second of a projected series of three volumes reviewing completely the chemistry of acetylenic compounds, presumably exclusive of the acetylenic hydrocarbons as such. Volume III is to cover the acetylenic carbonyl compounds and miscellaneous acetylenic compounds. Volume I: "The Acetylenic Alcohols" was reviewed in THIS JOURNAL, 25, 407 (1948). The comments made there in general apply also to Volume II.

Volume II, like Volume I, is clearly written, well indexed, thoroughly documented, and minute, and often critical, in its coverage of the literature. The main text surveys the literature through 1948, and an appendix covers most, at least, of the publications of 1949.

This series should be useful both to specialists and to those who desire general information.

LAWRENCE H. AMUNDSEN

UNIVERSITY OF CONNECTICUT
STORRS, CONNECTICUT

• ORGANIC SYNTHESES. VOLUME 30

Editor-in-Chief: *Arthur C. Cope,* Professor of Chemistry, Massachusetts Institute of Technology. John Wiley and Sons, Inc., New York, 1950. vi + 115 pp. 4 figs. 15.5 × 23.5 cm. $2.50.

VOLUME 30, the latest in the Wiley Organic Syntheses Series, was published in September. The book gives the most convenient laboratory methods for preparing various organic chemical reagents in one-half-pound to five-pound lots, each method being adaptable to large-scale development. This volume, like previous ones, gives the equations for the reactions, procedures for production with accompanying notes, and other methods of preparation for 39 different organic compounds. The directions have been contributed by 57 different collaborators in addition to members of the Editorial Board. The suggested directions have been further checked or verified by competent references. Preparations are listed alphabetically under common name and *Chemical Abstracts* indexing names. The subject index comprises material from this volume only, since all previous preparations are listed in Collective Volumes I and II, or Volume 29.

The present volume contains data on the following additional syntheses: 9-acetylanthracene; 3-aminopyridine; *p*-aminotetraphenylmethane; DL-aspartic acid; benzoylcholine iodide and chloride; *n*-butylacetylene; β-carbethoxy-γ,γ-diphenylvinylacetic acid; chloroacetonitrile; *trans*-2-chlorocyclopentanol; 4,4′-dichlorodibutyl ether; diethyl *cis*-Δ^4-tetrahydrophthalate and diethyl *cis*-hexahydrophthalate; 1,4-diiodobutane; ethanedithiol; ethylenimine; 5-ethyl-2-methylpyridine; ethyl phenylcyanoacetate; fumaronitrile; glutaric acid; hexahydro-1,3,5-tripropionyl-*s*-triazine; 2-iodothiophene; 2-mercaptobenzimidazole; methanesulfonyl chloride; N-methyl-2,3-dimenthoxybenzylamine; 1-methyl-3-ethyloxindole; methyl β-thiodipropionate; 1-naphthaldehyde; *o*-nitroacetophenone; phenylacetylene; *trans*-1-phenyl-1,3-butadiene; α-phenyl-α-carbethoxyglutaronitrile; α-phenylglutaric anhydride; phenylsuccinic acid; 2,3-pyrazinedicarboxylic acid; 1,2,3,4,-tetrahydrocarbazole; *cis*-Δ^4-tetrahydrophthalic anhydride; tetraphenylarsonium chloride hydrochloride; *o*-tolualdehyde; vanillic acid; vinyl laurate and other vinyl esters

Since this series began in 1921 these annual issues have become a necessary part of every chemical library here and abroad. This latest volume, for all practical purposes, is identical in quality, form, appearance, and workmanship to other previous members of the series. Many organic chemists will be glad to note that we can soon expect Collective Volume III of this important compilation.

RALPH E. DUNBAR

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FARGO, NORTH DAKOTA

● A GERMAN-ENGLISH DICTIONARY FOR CHEMISTS

Austin M. Patterson, Professor Emeritus of Chemistry, Antioch College. Third edition. John Wiley and Sons, Inc., New York, 1950. xviii + 541 pp. 13.5 × 18 cm. $5.

THIS dictionary, which first appeared in 1917, has become part of the standard furniture of a chemical laboratory. The third edition is nearly twice the size of the first. It has been brought thoroughly up to date and follows the latest nomenclature practices.

The introduction, which has remained practically unchanged through three editions, simplifies some of the difficulties of translation and is a valuable help to one who feels himself confused by the strange intricacies of a foreign language.

● GERMAN-ENGLISH TECHNICAL AND ENGINEERING DICTIONARY

Louis De Vries, Professor of Modern Languages, Iowa State College. McGraw-Hill Book Co., Inc., New York, 1950. xiv + 928 pp. 16 × 23.5 cm. $20.

THIS is a comprehensive dictionary giving exact definitions of technical terms in common use in the various engineering fields and in chemical technology.

It includes terms used in such industries as aluminum, automobile, motion pictures, petroleum, iron and steel, paper, textiles, and many others.

Numerous outstanding technical librarians and educators assisted in its preparation.

● PRINCIPLES OF NUCLEAR CHEMISTRY

Russell R. Williams, Jr., Assistant Professor of Chemistry, University of Notre Dame. D. Van Nostrand Company, Inc., New York, 1950. ix + 307 pp. 81 figs. 25 tables. 15 × 22 cm. $3.75

THE term "Nuclear Chemistry" in the title of the book is used in its broadest sense, that is to include those topics sometimes more restrictively classified as nuclear chemistry (reactions of nuclei and the properties of resulting nuclear species), tracer chemistry, and radiation chemistry. The author states in the preface that the book "is intended to serve as the basis for a one semester lecture course for advanced undergraduates and graduate students." As such it will undoubtedly be welcomed by many teachers responsible for giving an elementary course in nuclear chemistry.

The first five chapters (slightly over one-half of the book) cover the subjects that are, strictly speaking, nuclear physics but which form a necessary background for an understanding of the chemical phenomena related to nuclear properties and processes. Topics such as nuclear structure, nuclear decay, bombardment reactions, etc., are presented from an empirical point of view and at times in a very abbreviated manner. The last four chapters deal with the chemical phenomena and include such subjects as the chemical isolation of unstable nuclides, chemical consequences of nuclear reactions, chemical effects of nuclear radiations, and analytical applications of nuclear chemistry.

The principal difference between this book and others covering the same general field is the placing of a greater emphasis on the chemical aspects of the subject. Many chemistry teachers will welcome this shift of emphasis and will find this book particularly suited to their needs. On the other hand, the reviewer is quite certain that there will be many who will feel that the background of "nuclear physics" given in the first five chapters is inadequate and would require extensive supplementary material, particularly for a course at the graduate level.

KENNETH STREET, JR.

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BERKELEY, CALIFORNIA

● MODERN CHEMICAL PROCESSES

By the Editors of _Industrial and Engineering Chemistry_ and technical staffs of cooperating industrial organizations. Reinhold Publishing Corp., New York, 1950. 222 pp. 21 × 29 cm. $4.

BEGINNING in February, 1948, one of the editors of *Industrial and Engineering Chemistry* has cooperated with members of the technical staff of some organization to describe comprehensively a new plant or process, replete with excellent illustrations and flowsheets. In this volume have been collected the 23 articles appearing in 1948 and 1949. Of these, six describe inorganic processes, and seventeen describe organic processes. All represent new, up-to-the-minute plants and processes. These articles should be useful to students to supplement the older and less complete material found in textbooks. This book, despite the repetition of material, will save much wear and tear on the journals and for itself is deserving of a place on the library shelf.

KENNETH A. KOBE

UNIVERSITY OF TEXAS
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INTERSCIENCE
new publications

THEORY OF ELECTRONS

By **L. ROSENFELD**, *Professor of Theoretical Physics, University of Manchester, England.*

1951. 6 x 9. 135 pages, 7 illus., numerous formulae. $2.25

This book is based on a series of lectures for advanced students. It discusses the foundation for an atomistic interpretation of the electrical, magnetic, and optical properties of matter. The discussion is kept in a classical plane, with indications of the modifications rendered necessary by quantum effects.

SELECTED TOPICS IN X-RAY CRYSTALLOGRAPHY
from the Delft X-Ray Institutes.

Edited by **J. BOUMAN**, *Delft, Netherlands.*

1951. 7 x 10. 400 pages, 200 illus. $11.00

The Delft X-Ray Institute is a unique organization insofar as its staff consists of representatives of fundamental research and of experts in the major branches of the applications of x-ray structure determination, such as metallography, biology, plastics, and rubber. This teamwork is reflected in the reports of research done in the various laboratories.

TECHNIQUE OF ORGANIC CHEMISTRY

Editor: **ARNOLD WEISSBERGER**

Volume IV: DISTILLATION

1951. 6 x 9. 696 pages, 293 illus., 97 tables. $14.00

Authors: Arthur and Elizabeth Rose, Arthur L. Glasebrook, Frederick E. Williams, Carl S. Carlson, John R. Bowman, R. Stuart Tipson, Edmond S. Perry, John C. Hecker.

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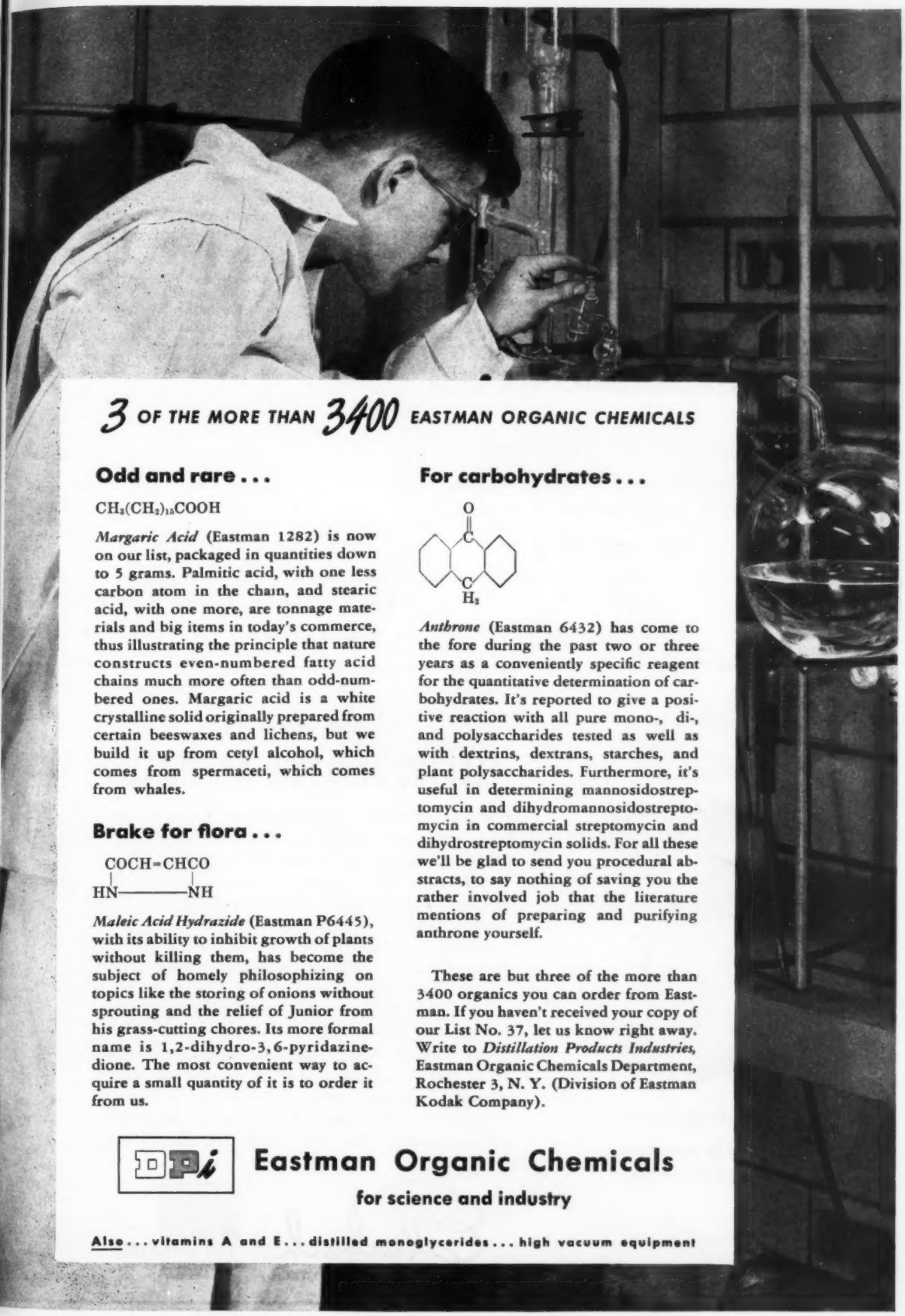

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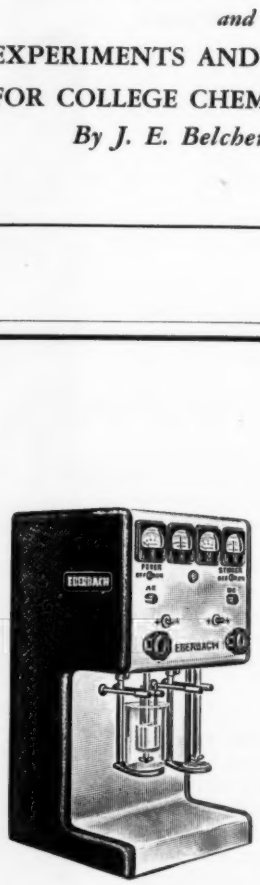

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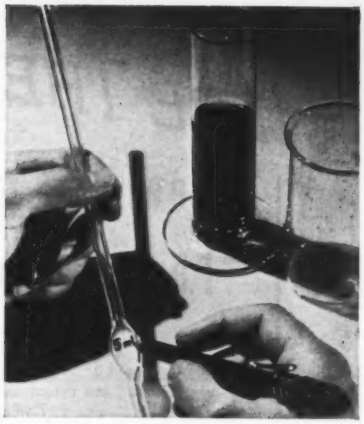

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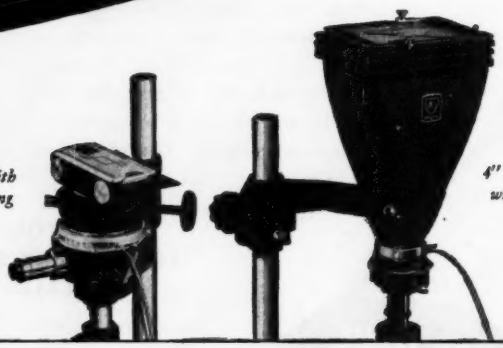

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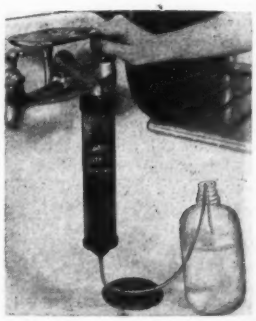

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[1] COLLEGE CHEMISTRY *by* Linus Pauling, *California Institute of Technology, 715 pp., 209 illus., $4.50, 1950.*

[2] COLLEGE CHEMISTRY IN THE LABORATORY *by* Lloyd E. Malm, *University of Utah, and* Harper W. Frantz, *Pasadena City College, 368 pp., 93 illus., $3.00, 1950.* (*Both books are illustrated by Roger Hayward.*)

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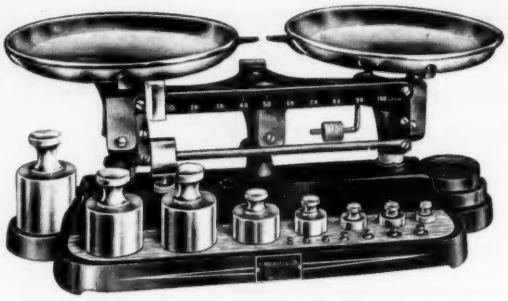

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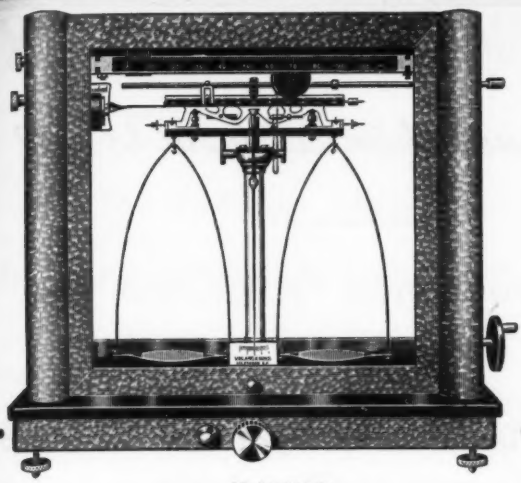

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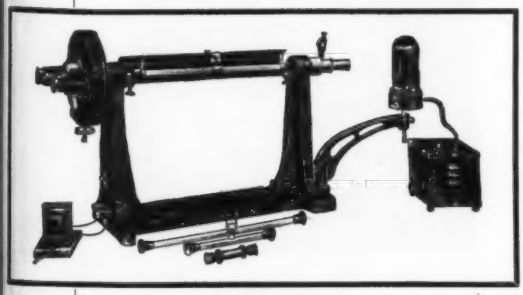

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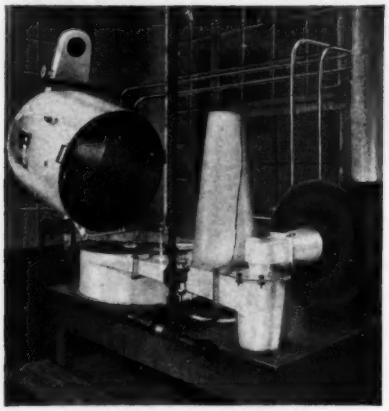

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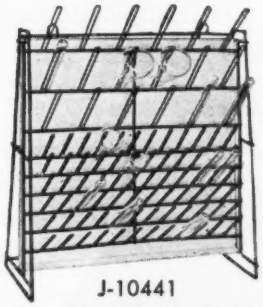

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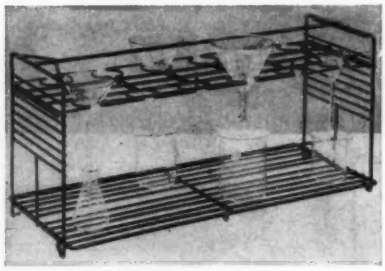

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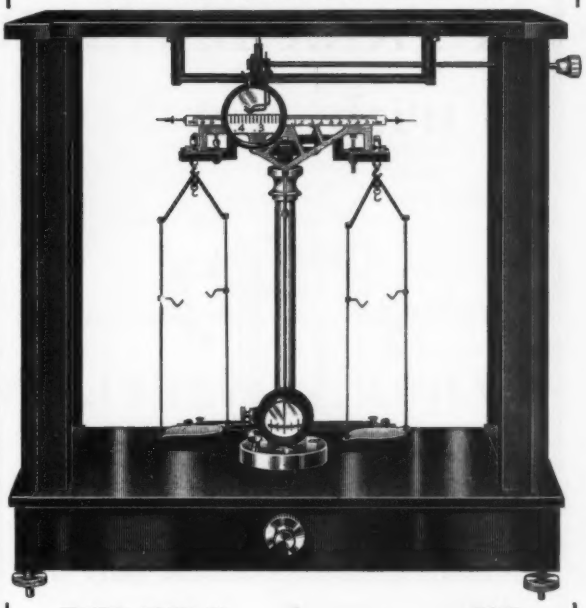

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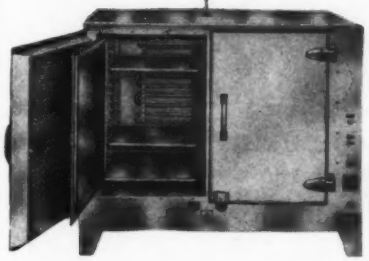

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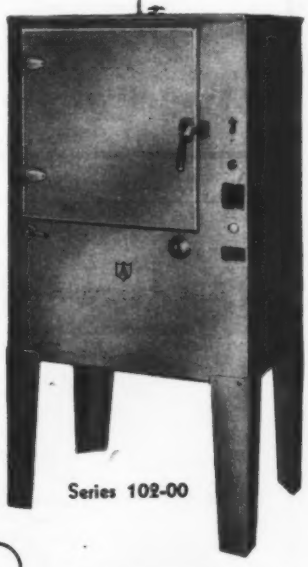

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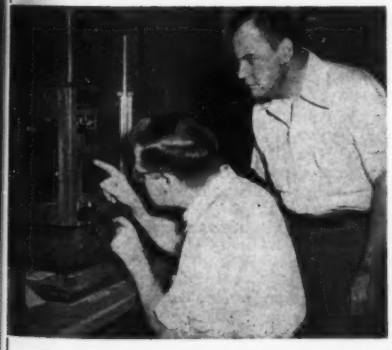

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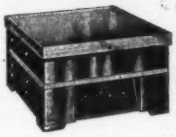

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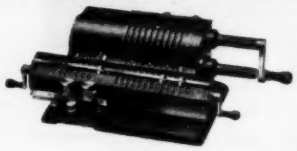

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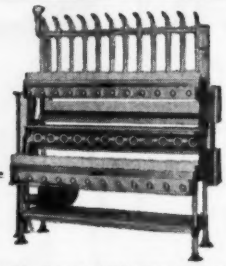

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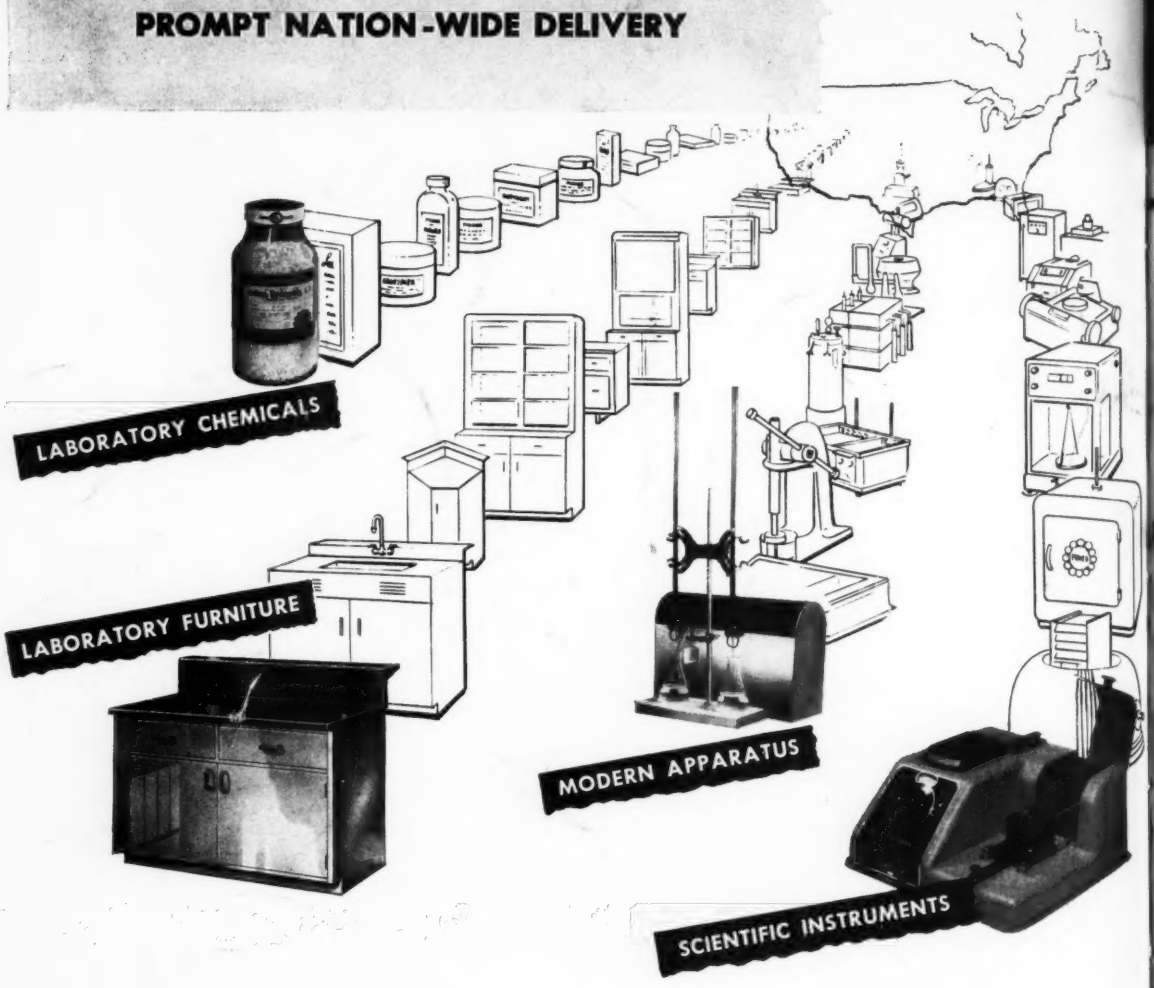

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